A

SERIES

OF

EXPERIMENTS

UPON

PHOSPHORI.

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BRITISH MUSEUM

ON

PHOSPHOR.

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EXPERIMENTS

RELATING TO

PHOSPHORI

AND THE

PRISMATIC COLOURS

They are found to exhibit in the DARK.

By B. WILSON, F.R.S. and Member of the ROYAL ACADEMY at UPSAL.

TOGETHER WITH

A TRANSLATION of TWO MEMOIRS, from the BOLOGNA ACTS, upon the ſame Subject,

By J. B. BECCARI, Profeſſor of Philoſophy at BOLOGNA.

LONDON,

Printed for J. DODSLEY in Pall-Mall, T. BECKET in the Strand, and G. KEARSLY in Fleet-Street; Meſſ. DOLLOND in St. Paul's Church-Yard, and Mr. SMITH in the Strand, Opticians,

M DCC LXXV.

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By J. B. BECCARI, Profess of Philosophy at Bologna.

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Printed for J. Dodsley in Pall-Mall, T. Becket in the Strand,
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MDCCLXIV.

A SERIES OF NEW EXPERIMENTS RELATING TO PHOSPHORI, AND THE PRISMATIC COLOURS They exhibit in the DARK.

THE Bologna ſtone, which has been ſo much ſpoken of for it's property of ſhining in the dark, and which was firſt diſcovered by accident, engaged the attention of many excellent chymiſts and experimental philoſophers to examine the nature of it's compoſition, and ſearch for the moſt advantageous method of calcining it, and making it luminous.

In conſequence of their diligent reſearches, we find very different and ingenious proceſſes in various

learned works: and particularly in the Berlin acts, by the celebrated M. Marggraaf; who in analyſing the heavy fuſible ſpar, and others of the like kind, produced ſome very fine phoſphori. But no one that I have ever yet met with has been more ſucceſsful, or has made a greater figure in experiments upon this ſubject, than the learned Beccari of Bologna.

This excellent obſerver, by great patience and induſtry, joined to very great abilities in philoſophical enquiries, made many new diſcoveries, which he publiſhed in two memoirs in the Bologna acts for the years 1744 and 1747, upon this ſubject: from which it appears that the family of phoſphori have been by him extended to an amazing number. Some ſhining with a greater, and others with a leſs light, after having been expoſed to the ſun for a few ſeconds, and then removed ſuddenly into the dark.

The moſt brilliant phoſphori which he diſcovered, were from linen, paper, ſome earths, ſtones, gums, and even the human ſkin: beſides others, when they were properly dried or roaſted. Thoſe memoirs containing ſo many curious facts, and this undertaking of mine being a kind of ſequel to his diſcoveries, I have thought it not improper to publiſh a tranſlation of them at the end of this work: as they may ſerve to illuſtrate each other, and render the whole of this enquiry more eaſy to be underſtood.

The

The apparatus employed by Beccari at firſt, for bringing bodies ſuddenly from the light into the dark, did not, as he acknowledges, anſwer the end required ſo well as the ſecond he made uſe of.

I have had the ſatisfaction of trying both methods, and found that the latter was, on many accounts, more eligible, and better calculated for the purpoſe. But as the duration of the phoſphoric light, mentioned by him, was ſhorter conſiderably than what I have obſerved, I am inclined to think that one difference, at leaſt, aroſe from a difference in the degree of darkneſs obtained, between the kind of box he made uſe of; and the ſmall cloſet I employed, the dimenſions of which being about ſix feet by five and a half.

It was painted black, or covered with black baize in every part; and had two doors that were five or ſix inches broader and longer than the ſpace to enter at. There were two curtains of black cloth over the hole where the hand was occaſionally put out, to expoſe bodies to the light; the outer one conſiſted of three doubles, and the inner one of as many. All theſe were conſiderably larger than the aperture, which was about fifteen inches diameter, and opened to the South. There were ſmall leaden weights faſtened to the bottom of each curtain, to preſerve them in their places when the hand was drawn into the room. And, that I might breathe the air freely, and continue longer in the cloſet without any inconvenience,

there were fixed two curving pipes, about three inches diameter; one communicating with the external air from the top of the room, and the other with the external air from the bottom. The cloſet, in which I made theſe experiments, was in the houſe of Edward Delaval, Eſq; which ſtands in an open ſituation on the ſide of the Thames, at Weſtminſter.

In my firſt experiments I adhered ſtrictly to the rule laid down by Beccari, of preparing the eyes for obſervation, by continuing in the dark a full half hour before an experiment was attempted to be made. But I now find that a much ſhorter time is ſufficient, at leaſt, for my purpoſe, and with my eyes; unleſs the bodies to be examined afford a very feeble light. Before I begun to make any new experiments, I repeated many of thoſe made by Beccari, and found that they anſwered, in general, as he hath related them. In the courſe of theſe trials my attention was chiefly engaged with ſuch of the phoſphori only as appeared moſt brilliant: they being the beſt calculated for my purpoſe to make further experiments upon; not only on account of their great ſplendor, but the duration of it. Of thoſe luminous appearances the experiments upon paper are not the leaſt conſiderable.

For Beccari, having obſerved that paper would ſhine pretty well in the dark, and with an equable light throughout, after it had been expoſed to the ſun,

ſun, tells us, that he laid a ſheet of it upon a gridiron, and underneath it placed ſome burning coals; that, when the paper had been heated enough, he expoſed it to the light, and then removed it into the place of obſervation, where he found the paper ſhining with an uncommon light: and that on thoſe parts of the paper which had been covered with the bars there was an appearance of darkneſs, which exhibited the outlines of the bars terminated with the greateſt preciſion.

The ſame ingenious author mentions afterwards another experiment, where he laid a pretty thick plate of braſs upon the paper, which had been previouſly made ſo hot, that it could hardly be held in the hand, but yet it was not ſo hot as to hurt the paper: when the plate had made the paper ſufficiently warm, he expoſed it to the light as before, and afterwards removed it into the dark. That part of the paper which the plate had touched appeared to him of an extraordinary brightneſs; and ſhewed the figure of the plate perfectly defined: the other part of the paper exhibited the uſual light, evenly diffuſed over every part of it.

This extraordinary effect of heat upon paper induced me to purſue it further. Accordingly I had recourſe to what I thought a more convenient method, as it ſeemed better calculated to vary the degrees of heat. This was nothing more than a common ſmoothing iron, ſuch as the women em-

ploy

ploy upon linen, into which I occaſionally put a red hot heater.

One day, in the month of Auguſt, when the ſun was ſhining, I placed the ſmooth ſurface of the heated iron on a quarter of a ſheet of white paper which laid upon a table, and continued it there for three or four ſeconds. After that, I placed the paper in the cylinder of the firſt apparatus deſcribed by Beccari, and in the manner he had done. On expoſing this paper to the ſun for two or three ſeconds, and then removing it into the dark ſuddenly, by turning the cylinder back again, a light appeared ſo very bright, that I thought ſome accident had happened to the apparatus; and that, in conſequence of ſome fracture, the light of the ſun had ſhone through it upon the paper. After examining every part, and finding that all was ſecure, I repeated the experiment many times, without warming the paper afreſh; when I was ſoon ſatisfied, that the extraordinary brightneſs on the paper was manifeſtly phoſphoric, and exhibited the exact ſhape of the iron. The other part of the paper ſhone alſo, but ſo faintly, that it looked like a ſhade compared with that ſplendid appearance.

Upon warming the paper again with the ſame iron, and repeating the experiment, by expoſing it to the light, I placed the iron upon a part of the luminous appearance for about two ſeconds. By this treatment, as much of the phoſphoric light as the iron covered, and no more was totally extinguiſhed.

guiſhed. This extraordinary effect, which does not appear to have been obſerved by Beccari, rather excited my attention; and ſtill the more ſo upon finding, that, when I expoſed the paper again to the ſun, the brightneſs was reſtored.

I therefore varied the experiment, by applying the ſame iron, when it was cold, to the luminous part of the paper. Accordingly, immediately after the light was renewed, I placed the iron upon the luminous part, and continued it there, in different trials, from two or three, to eight or ten ſeconds, without having any effect upon the light.

I had then the curioſity to try the effects of other bodies upon the illuminated paper, when they were in the ſame ſtates both as to heat and cold. And firſt with reſpect to heat.

Having heated a ſmall piece of gold, I laid it in the middle of the luminous part of the paper for two or three ſeconds; and after removing it I obſerved a dark ſpot exactly the ſize of the gold, which became luminous again by expoſing it to the light.

Silver, and other metals, when they were made equally hot, produced the ſame effect.

I then heated a piece of marble; and after that, a ſtone of the grit kind; alſo glaſs, and two or three different ſorts of wood; each of thoſe had the ſame effect upon the luminous part of the paper, as the heated iron, &c.

I now repeated the experiment with all theſe ſubſtances, when they were in a cold ſtate, but not

not one of them had any effect upon the luminous appearance.

Now as all thoſe bodies were applied in contact with the paper, I varied the experiment again, by ſuffering thoſe bodies to approach no nearer to the luminous part of the paper than about the 30th part of an inch.

For this purpoſe I provided myſelf with a circular rim of braſs, which was open in the middle. This I laid upon the phoſphoric light of the paper, and then placed the heated iron upon it; and though I varied the times of continuing the iron upon the braſs, yet it had no effect upon the luminous part; except where the braſs was in contact with the paper, and there it appeared more or leſs dark in proportion to the heat which the different parts of the braſs had acquired from the iron.

Having now ſeen the effects of different applications of heat upon the phoſphoric light, I was ſollicitous to obſerve the effects of fluids, when in the ſame ſtate, upon the ſame luminous appearance.

To this end, whilſt the paper continued ſhining, I dipped a part of it in boiling water. This treatment preſently extinguiſhed the luminous appearance. Upon expoſing the paper again to the ſun, though in it's moiſtened ſtate, the ſplendor revived: but then it was not quite ſo bright, as before it's immerſion in the water.

In very hot oil the phoſphoric light did not diſappear ſo ſoon: on expoſing it again to the ſun, the

the paper ſhone ſomething brighter, than when moiſtened with water.

Theſe different fluids, in their cold ſtate, had but little effect upon the luminous appearance, or upon the duration of it. And I find ſince, that the learned Mr. Du Fay had obſerved the ſame thing, by plunging other ſubſtances in cold water.

In the making of theſe experiments, I was a little ſurprized with a ſingular appearance. For when the hot oil was brought into the cloſet, where I had continued for a conſiderable time, I obſerved that the oil, as well as the baſon which held it, were both diſcernible; and likewiſe ſeveral objects that happened to lie near, without the aſſiſtance of any light from without.

Beſides this ſingular appearance I made another obſervation upon an effect the heated iron produced: though I have found ſince, the learned Marggraaf has deſcribed the ſame appearance which he had obſerved with the fuſible ſpar, when expoſed to a heated plate of iron.

It was this: after the iron had made the luminous part of the paper dark, I continued it upon the paper for eight or ten ſeconds more. In conſequence of this, a new phoſphoric light was produced, without expoſing the paper afreſh to the ſun. The brightneſs of this light was about equal to half of that phoſphoric light which the ſun produces on paper. On examining the paper in the open day, there appeared only a very tender yellowiſh tinge in thoſe parts where the iron had

been in contact. And perhaps it may be worth while to obſerve, that this paper, notwithſtanding the tender tinge, would ſhine very well in the dark, upon preſenting it again to the ſun.

But I found that whenever the paper happened to be burnt conſiderably darker, ſo that it bordered upon blackneſs, it would not produce any light by being expoſed to the ſun.

Dr. Prieſtley, in his hiſtory of Beccari's diſcoveries, has mentioned a very remarkable experiment. He tells us that Beccari found, that paper, after it had been made *red hot*, and *cooled* again, was an excellent phoſphorus *. I muſt own, that upon the ſtricteſt reſearch into the work to which he refers, I have not been able to find any ſuch account. Nor do I conceive in what manner paper can have been made red hot, and afterwards cooled, without being reduced to *aſhes*. I ſhould nevertheleſs be greatly obliged to the learned hiſtorian who relates the experiment, for an explanation of his meaning, if he can point out the paſſage to which his elaborate work refers.

Linen,

* " Beccarius himſelf ſuſpected that many other changes in " the internal ſtructure of bodies were made by the light of the " ſun, and he was ſatisfied that ſeveral ſubſtances which had the " property of imbibing light, were much injured by them in " that reſpect. He found that peper, after it had been made red " hot and cooled again, was an excellent phoſphorus of this " kind; but he was ſatisfied, notwithſtanding his firſt ſuſpicions " to the contrary, that it was greatly injured by being expoſed " to the light."

Dr. Prieſtley's Hiſt. of Viſion, Light, and Colours, p 382.

Linen, treated in the ſame manner as the paper was in all the preceding experiments, afforded very nearly the ſame appearances: except that the phoſphoric light upon ſome fine linen was conſiderably brighter than upon a coarſer kind; and that the duration of the light was very near twice as long. And yet I have met with ſome inſtances where exceeding coarſe linen has afforded a very bright light, which laſted full as long as the finer ſort.

I now laid aſide the cylinder, upon finding ſeveral inconveniencies ariſing from the uſe of it: and in it's place I put the other apparatus, conſiſting of the black curtains only. This I continued to employ during the whole time of my reſearches: but with this difference, that I was obliged to ſhut my eyes at all times whilſt I expoſed the bodies to the ſun, on account of the light which entered the cloſet by the unavoidable opening of the curtains.

Sugar not only ſhines very well in the dark, but may be made a much finer phoſphorus by a proper application of heat. I found this by expoſing it in contact with the ſmooth part of the heated iron; which was not hot enough to melt the ſugar, but only ſufficient to leave a whitiſh gloſs upon it's ſurface.

I endeavoured to increaſe this brightneſs further by a variety of means, and particularly by melting very ſmall quantities thereof upon a card, with the aſſiſtance of the heated iron; but they did not anſwer my expectations. However, my frequent attempts produced a very ſingular effect,

which in it's conſequences turned out to be of conſiderable uſe in my reſearches.

The ſmoothing iron I made uſe of, by repeated experiments, and thoſe for ſeveral days together, was grown foul with the ſugar, and perhaps other matter, ſticking to it; ſome of which were melted, and others not. I nevertheleſs continued to uſe it in the ſtate it was. And one day, in preſſing the iron upon a card where a little powdered ſugar had been previouſly laid, and which was melted by the heat from the iron, I obſerved, after drawing the card in from the light, a moſt beautiful *green appearance* in ſeveral parts of the card, and exceedingly brilliant; it reſembled moſt the colour of a very fine emerald in the light of the ſun. Perhaps the novelty of it might prejudice me, and induce me to think it brighter than it really was. The duration of it was as long as the white light in other parts of the card. When the green colour vaniſhed, I renewed it many times, and for four or five days afterwards, by only expoſing the card again to the ſun.

Several times afterwards I made the ſame experiment upon two or three other cards with equal ſucceſs. But I ſoon found that it was not always in my power to produce it, notwithſtanding I made uſe of every means I could think of in numberleſs attempts. All theſe trials ſatisfied me, that the green, produced above, muſt have depended upon ſome circumſtances I am not yet acquainted with.

Beccari

Beccari has obſerved, that ſugar would ſhine, not only upon it's ſurface, but within it's ſubſtance throughout. I found the obſervation true, by breaking it, and even pounding it; for all the parts continued equally luminous.

I then plunged in cold water ſome freſh enlightened lumps, without being able to obſerve any alteration in their ſplendor. The duration of this light in the water was nearly as long as that from other lumps which were out of the water.

In this place I muſt obſerve, that the phoſphoric light ceaſes before the ſugar is intirely diſſolved; and though hot water diſſolves ſugar much ſooner, I could nevertheleſs perceive a light for two or three ſeconds; and this appearance I conjecture was owing to the water, and perhaps the heat, not being able to penetrate it immediately.

White ſugar-candy ſhines alſo in cold and hot water, after it has been expoſed to the light, and for a longer time reſpectively. And though loaf-ſugar, and ſugar-candy, have been immerſed in cold water for a few ſeconds, they will, even in their moiſt ſtate, ſhine afreſh upon expoſing them again to the ſun.

Among a variety of experiments upon vegetables, Beccari diſcovered that bread, when it is a little toaſted, ſhines very well. But I do not find that the light thereof penetrates farther than the ſurface of it; becauſe, on breaking it, the new ſurfaces are totally dark. Bread I find will ſhine in the dark without being toaſted; but then

the

the light is rather weak, and that may be the reaſon why Beccari had not obſerved it.

The light which gum arabic affords anſwers fully Beccari's deſcription of it. Gum copal ſhines leſs than gum arabic, except when it is very well dried by a fire, and then I found it to be a very good phoſphorus. Both theſe gums I obſerved ſhine nearly as well in cold water, and almoſt for as long a time as in air.

Cotton is no inconſiderable phoſphorus; at leaſt, it is as good as paper without the application of heat.

HAVING finiſhed what I had to obſerve upon the vegetable kingdom, we ſhall now attend a little to the animal kingdom.

I find that common joiners glue ſhines conſiderably better than bread untoaſted; but, upon expoſing glue to the fire, till it was extremely well dried, and ſo as to ſwell and change it's colour to a lightiſh yellow, I obſerved it gave a very fine yellowiſh light, which would ſhine in cold water for a conſiderable time, and nearly as bright as in air.

There are ſome bones that ſhine much better than others, and eſpecially if they happen to be properly dried: at the ſame time I muſt obſerve, that there are others which do not afford any light that I could perceive.

Hair, and wool, are alſo ſubject to ſhine, though not in any great degree; but even theſe differ alſo in that reſpect; for ſome hair, as well as ſome wool,

ſhine

ſhine better than others: but wool in general ſhines conſiderably brighter than hair.

Feathers give a little phoſphoric light; the white appear the beſt; the yellow ones next; red and green the leaſt; but the quill part ſhines in general better than even the white of the feathers.

There are few ſhells but what ſhine, and ſome ſo conſiderably, that they exceed the light which paper affords: for example, oyſter-ſhells, cockles, and what is called the ſkuttle-fiſh.

I have tried a great many other ſubſtances in the animal kingdom, but I decline giving any account of them, as Beccari hath obſerved the ſame, in a very able manner, and many more that are exceedingly curious.

IN regard to the mineral kingdom, Beccari has given us a great variety of very fine phoſphori. His diligence has been ſo great in this diviſion of nature, that I had very little hopes of adding any thing conſiderable, which might be worthy of notice. However, I was determined to purſue my obſervations upon ſuch ſubſtances as were the moſt phoſphoric: but not without ſome expectations that chance, and induſtry, would in time diſcover ſomething more deſerving my attention, than barely ſeeking for the moſt brilliant phoſphori.

Contrary to Beccari's plan, the firſt object I had in view was to examine the gems; notwithſtanding they had been diligently attended to by him, Du Fay, and others.

Among

Among thoſe that are phoſphoric, there are many diamonds, and ſome of different colours, which ſhine very well, and appear lucid throughout. There are others, and thoſe very fine, that give little or no light, which agrees with the obſervations made by Mr. Boyle, Du Fay, Beccari, &c.

However, I had a ſingular opportunity, by the favour of Lord Pigot, of examining in the dark his exceeding fine diamond, it being the moſt valuable one in this kingdom from it's water and ſize, and weighing two hundred grains. The great luſtre and magnitude of it, though *unſet*, were flattering circumſtances that it would produce a conſiderable phoſphoric light. But our expectations were greatly diſappointed by making the experiment. For inſtead of perceiving a brilliant phoſphoric appearance, we only were able to obſerve light enough from it to pronounce it's preſence, after it had been expoſed to the ſun. The degree of this light did not exceed that produced from the red and green feathers mentioned before.

The ſame noble Lord, who was extremely obliging and ready to promote the enquiry I had in hand, produced a large brilliant drop, which was alſo unſet. This diamond gave no better light than the former.

But a large yellow diamond, *ſet* in a ring, and which belonged to the ſame nobleman, produced a very good light, that laſted ſeveral minutes.

Some

Some diamonds of leſs value, which were alſo ſet, and ſome of different colours, were phoſphori: but ſome ſhone better than others.

Two rubies, a ſaphire, topaz, and aqua marine, which were very large and fine of their kind, gave no light.

The opal gave a pretty good light, but a fine emerald only an indifferent one.

The cat's eye made as indifferent an appearance: but a very large pearl in Lord Pigot's collection ſucceeded better: the light of which being nearly equal to that which paper gives without heat, and the duration of it was about twelve ſeconds.

Beſides theſe gems, I was favoured with ſeveral more of conſiderable value by Lord Seaforth: and particularly, a yellow diamond that was ſet tranſparently, which gave a moderate light, though it rained heavily at the time of obſerving it. I mention the circumſtance of tranſpareney, becauſe the yellow diamond belonging to Lord Pigot deſcribed above, had a *foil underneath it*.

MARBLES have been obſerved by Beccari to ſhine with a conſiderable degree of light: but I find that Portland ſtone, and a whitiſh ſtone of the grit kind, which is very common, and found in England, ſhine a great deal better than marbles, or even alabaſter, or lime-ſtone.

Some mortar from an ancient part of the Tower of London exceeded the grit ſtone, and even chalk, though it had been in the fire for ſome time. The

light diffuſed over the ſurfaces of thoſe bodies did not appear to penetrate any farther: for, upon ſcraping or ſcratching them, there was a total darkneſs underneath.

Flints, I found, differ very much in the degrees of phoſphoric light they afford; the cleareſt kind having the advantage: and thoſe ſeem to ſhine internally as well as externally. I have made the the ſame obſervation upon many ſpars and ſome cryſtals.

A very fine ſpecimen of ſpar from Derbyſhire, of a cubic form, ſhone as bright as ſugar in it's improved ſtate by heat. And when I plunged it in cold water, the light continued very nearly as long as in air. But in hot water this was not the caſe: for the light diſappeared in a quarter of the time. This ſpar, beſides many others, as well as various other ſubſtances, and eſpecially if they happen to be of an inflammable nature, ſhine in the dark, merely by the application of heat. It is the ſame with a kind of greeniſh ſpar or fluor from Sweden. This fluor, expoſed to the light, ſhines full as well as ſugar in it's common ſtate. But what appears remarkable, is, that though it may not be one of the moſt brilliant phoſphori, yet it's light continues above ſix minutes. This ſeems contrary to Beccari's rule, which ſays, that bodies ſhine the longer, the brighter they are: now ſome of the brighteſt phoſphori that I have hitherto obſerved, do not laſt above thirty ſeconds.

Tutty,

Tutty, Beccari obſerves, will ſhine on the convex, and not on the concave part; I mention this laſt circumſtance as I find the concave part does give a light; but then it is a feeble one compared with the other.

White arſenic affords a light almoſt as bright as paper in it's common ſtate.

Corroſive ſublimate gives a weaker light conſiderably.

The flowers of zinc produce a better light than corroſive ſublimate: and ſometimes the colour of it is inclinable to the orange.

IN examining all the calcareous bodies I could think of, I obſerved in general, that they produced about a middle degree of light between the brighteſt and fainteſt phoſphori I have yet met with. The light of moſt of them, however, was increaſed by calcining them in a crucible; and in ſome more than in others: particularly lime-ſtone, chalk, and oyſter-ſhells. Among the laſt I ſometimes imagined there were faint appearances of colours. In two or three ſpecimens I am certain of having obſerved an orange colour, and a very tender blue. But not meeting with the ſame appearance on others, after repeated experiments, I gave the purſuit over for that time.

AMONG the neutral ſalts Beccari mentions borax as ſhining with a conſiderable degree of light, but has not told us, whether it was refined or not. The phoſphoric light from either the native or refined borax, I have not found the leaſt remarkable. The latter has the advantage over the other:

but even that, is not brighter than what ſugar affords without the application of heat.

When borax is calcined, the light is a good deal improved: but not near ſo much as when it is calcined with chalk. The ſplendor this compoſition affords, when it is properly made, is very great indeed; and very near being equal to the moſt brilliant phoſphori I have yet met with.

It was after a variety of trials, and without ſucceeding, that I thought of putting a lump of borax into an iron ſhovel, and afterwards a large ſurface of a lump of chalk upon the borax while it was melting. In a little time part of the chalk incorporared with the borax, and formed a kind of calx, which ſpread from under the chalk in large feather-like forms, curling upwards. The heat was continued only till all the borax was reduced to a calx. The upper ſurface of this calx was the part that ſhone ſo bright. The under ſurface being conſiderably leſs bright: and the chalk itſelf, that was untouched by the borax, gave only the light that is common to it, which appeared as a pretty ſtrong ſhade, compared with the very great ſplendor of the calx on it's upper ſurface.

After this I combined borax with many other ſubſtances, ſuch as gypſum, oyſter-ſhells, lime-ſtone, ſulphur, &c. but the light they afforded was not near ſo bright.

I then examined the ſedative ſalt, both by calcination and ſublimation: as likewiſe the foſſil

alcali.

alcali. Each of thoſe gave a light nearly equal in point of brightneſs: and very near equal to borax calcined alone.

In the courſe of theſe experiments I was obſerving the phoſphoric effects of different pieces of amber, ſome of which were poliſhed and others not. The poliſhed ones gave a much better light than the others; but even thoſe were not quite ſo bright as paper without the application of heat. And having the heated iron at hand, I placed one of the poliſhed pieces upon it. In a very little time a reddiſh intenſe light appeared to dart from one end of the amber to the other: but this light ſeemed inſtantaneous, and reſembled very much the electric light. At the ſame time there was a kind of crackling noiſe attending it: which created a ſuſpicion that the amber had been injured by the heat. But, upon examining it in the open day, I could not perceive the leaſt alteration.

I repeated this experiment with a large lump of amber, which was not poliſhed, and obſerved an appearance very like the former; but then this light did not extend ſo far; owing I ſuppoſe to the irregular ſurface not permitting the iron to be in contact with it to ſo great a length.

WHILST

WHILST I continued buſy in making theſe kind of experiments, and with other ſubſtances that were of an unctuous and ſulphureous nature, I ſtill kept in my remembrance the extraordinary green light which had been obtained from ſugar. The weather being ſettled, and very fine, I attempted once more to try whether I could not ſucceed in making it by ſome other means. And as I knew that copper, in certain circumſtances, would produce a green colour; I naturally had recourſe to that ſubſtance, notwithſtanding the diſcouragement I met with from the experiments and obſervations made by Beccari and others upon metals in general: and though I had no pretenſion to any ſkill in chemiſtry, I had a mind to try what a ſolution of copper in aqua fortis would do, when united with a calcareous ſubſtance.

Sol. of Copper. Accordingly, I put a ſmall quantity of aqua fortis, not exceeding five drops, on a piece of copper, and continued it there near twenty ſeconds. I then waſhed the ſolution off ſuddenly with half an ounce of freſh aqua fortis. Upon one ounce and a half of calcined oyſter ſhells, part of which happened to be in powder, and part not, I poured the aqua fortis thus impregnated with copper, and let it ſtand about ſixteen hours; not for any particular reaſon, but that it happened to be moſt convenient for me on account of other buſineſs. I then poured off what the oyſter-ſhells had not taken up, and put the ſhells, which were in a paſte-like form, into a crucible, and preſſed the maſs down very cloſe. After this,

I put

I put the crucible into a ſea-coal fire, which was pretty hot, and continued it there near forty minutes. On taking it out, and letting it cool, the whole maſs came eaſily out of the crucible, and appeared of a dirty greeniſh hue externally. This maſs I preſented to the light ſeveral times, and drew it into the dark as often, without obſerving any thing more than a weak, greyiſh, dirty light; though every part of the ſurface was properly expoſed, and carefully examined. I then broke a little off from the maſs at the end, which had been next the bottom of the crucible, when there appeared a largiſh lump of oyſter-ſhell, and more white than any other part in the open day. Upon preſenting this bottom part to the external light, and bringing it back again ſuddenly into the dark, I was exceedingly ſurprized with a general appearance of colours, reſembling thoſe in the *rainbow*, but far more vivid. The red appeared the fineſt, and not unlike that which we meet with in old painted windows when the ſun ſhines upon it. The yellow was not near ſo intenſe, but very bright, and lay next the red. The green next; but rather fainter than the yellow; and yet it was bright, though far ſhort of the green which I produced from ſugar. The blue was a good deal fainter, and not near ſo bright as the other colours. In regard to the purple I had ſome doubts, and therefore will not inſiſt upon it's being there.

The whole ſpace thoſe colours occupied was more than half an inch in length, and about one quarter in breadth. In

In conſequence of this curious and accidental appearance of the colours, I redoubled my induſtry to diſcover the cauſe of it.

The firſt ſtep I took was to examine the parts of the calx, where the colours appeared with a magnifying glaſs, and particularly thoſe next the confine of the red, where they were in greater abundance than in any other part. In making my obſervations, ſome parts that gave the phoſphoric red crumbled away by the ſlighteſt touch; for theſe obſervations were made on the ſixth day after the colours appeared; and which probably would not have been the caſe on the firſt day, the whole maſs being then firmer and more compact.

Some of theſe colours continued ſix days at leaſt, but not near ſo diſtinct as on the 1ſt and 2d days; for the yellow, green, and blue, were conſiderably fainter than the red. On the 8th day they had all vaniſhed, and a white, yellowiſh light occupied their place; and this light was rather brilliant than otherwiſe. I have been very circumſtantial in deſcribing this experiment, becauſe I apprehended a great deal would depend upon it. On the loſs of this curious ſpecimen I was very induſtrious to replace it by repeating the experiment. But experience ſoon taught me that it was not ſo eaſy as I at firſt apprehended.

However, in my ſecond attempt I took an equal quantity of the ſame ſolution, and poured it upon an equal quantity of oyſter-ſhells from the ſame parcel I had taken the former. The maſs, after being preſſed down in the crucible as before, was placed

placed in the fire for an equal time, and then expoſed to the light, after I had broke a little off from the bottom of the maſs, as I had done in the other experiment, but no colours appeared; and what light there was ſeemed rather faint, and of a greeniſh duſky white. I then broke the whole maſs into a number of parts, and obſerved the ſame hue throughout.

I varied the experiment many ways without ſucceſs. Sometimes the light from different maſſes in the different experiments appeared in colour like the new ſurfaces of broken ſteel. At laſt I begun to imagine, that larger ſolid parts of the ſhells might perhaps be neceſſary to produce the appearance of colours. But before I made the trial, I had an inclination to ſee what effect other metals would produce when treated in the ſame manner as I had done the copper.

In one ounce of aqua regia I diſſolved one leaf of gold, and poured the ſolution upon ſome of the ſame calx. The maſs was preſſed down in a crucible, and expoſed to a pretty ſtrong fire for ten minutes. On taking the maſs out of the crucible, the whole lump appeared of a purpliſh hue by day light, and gave only a very little phoſphoric light, rather inclining to the purple. I then broke it into ſeveral pieces, notwithſtanding which, every piece had the ſame purpliſh hue in the open day, except two or three ſolid lumps, about the ſize of a pea; and thoſe appeared more white, and freer from the purple tinge. On ex-

Sol. of gold.

poſing thoſe to the light, and drawing them back into the dark, a yellowiſh white light appeared; and round about them, to a ſmall diſtance, the light was fainter; but it had a little red and bluiſh hue. The general effect upon my eye, at a greater diſtance, was a kind of tender flame colour.

Theſe pieces becoming moiſt in three or four days, I expoſed them again to the fire for ten minutes more. By this treatment, thoſe ſolid parts gave a very good red throughout, no traces of the other colours being left. But this red was not near ſo intenſe as the former one with copper.

Upon expoſing thoſe ſpecimens in the fire for ten minutes more, all the red appearance vaniſhed, and a paliſh white light ſucceeded.

Sol. of ſilver. A ſolution of a leaf of ſilver in aq. fortis, mixed with powdered ſhells as before, produced, in a ſolid part of the ſame ſhells, a faint reddiſh purple and a little yellow, with more blue than yellow. The general effect of thoſe colours at a greater diſtance, was not unlike the opal: but it did not laſt above twelve ſeconds. After that a pale bluiſh white light took place.

Sol. of copper. Theſe laſt experiments gave me ſome further encouragement to try *larger* pieces of ſhells. Accordingly, having procured ſome calcined ſhells that were pretty thick, I broke them into convenient pieces for the purpoſe of putting them into a ſmall crucible. I preferred having *ſeveral* of thoſe pieces at the ſame time in the crucible, that

that I might have the greater chance of obtaining the colours I was in ſearch of. Upon theſe pieces of ſhells I poured a ſimilar ſolution of copper, and expoſed them in the fire about twenty minutes. The ſeveral pieces were afterwards examined in the dark, after they had been expoſed to the ſun. In conſequence of employing thoſe ſolid ſhells, I obtained a view of the object I ſo eagerly had ſought after: for there appeared a variety of fine ſpecimens of colours on moſt of thoſe ſolid pieces. The red and blue appeared moſt lively; next to them the yellow and green: as to the purple, it was ſtill very doubtful.

Cal. ſhells.

Leſt I might be led into any error in thoſe appearances from the ſhells themſelves being the cauſe of them, without the addition of any foreign mixture, I examined a large parcel of them, that had been properly calcined in a crucible with a view to other experiments. Among thoſe I found a few, as I have obſerved before upon another occaſion, that did give a faint appearance of priſmatic colours; but the red rather inclined to the orange. Theſe I laid aſide, and made uſe of ſuch only as gave a white light.

It may not be improper to add, in this place, an obſervation or two that I made reſpecting the crucibles, in which the ſhells were calcined, and upon the ſhells themſelves after calcination. By ſome accident, or perhaps by the fire itſelf, the crucibles were ſometimes cracked. The ſhells, in this caſe, were generally more coloured than

when the crucible was not injured. Another obſervation was, that thoſe edges and corners of the ſhells, which happened to touch the ſide of the crucible, frequently gave an orange coloured phoſphoric light. The novelty of this curious appearance did not, however, interrupt the other experiments I had in view.

I gave a preference to calcined oyſter-ſhells for the baſis of my work, in conſequence of a great variety of experiments, which had been previouſly made upon many other calcareous bodies.

Sol. of gold. A ſolution of half a leaf of gold in one ounce of aq. regia, was put upon ſome pieces of ſhells in a crucible, and continued in the fire for twenty minutes. The ſhells acquired the purpliſh hue mentioned before, and gave a very little phoſphoric light in the dark, except in a few parts, which had not partaken of the purple hue; and on thoſe parts there appeared a little red and bluiſh light. I was ſomewhat diſappointed in this experiment, as I expected to have found the colours not only broader and brighter, but more in number. I had therefore the curioſity to remove the purple hue, by ſcaling the ſhells tenderly with the point of a penknife. After this the new ſurfaces were expoſed to the light one by one; and then examined in the dark; when a bright red, yellow, green, and blue, appeared on ſome, and only red and blue on others. Two of them were faint, and exhibited a kind of flame colour.

This laſt experiment was repeated, but with a ſolution of ſilver in aq. fortis. The colours here were more vivid than with gold; particularly the red and blue. Whether any difference in the degree of heat, employed in the two experiments, (the duration of it being nearly the ſame) was the cauſe of the difference in their colours, I cannot tell; or whether it might not ariſe from ſomething elſe which eſcaped my obſervation. Theſe are circumſtances however, which may ſome time or other be aſcertained by others: my deſign, in theſe reſearches, being only to relate the facts as I really found them. Sol. of ſilver.

A ſolution of iron with the ſhells produced the colours alſo; but the blue appeared by far the moſt brilliant. Sol. of iron.

A ſolution of tin gave but a faint appearance of the colours, excepting the green, which was rather vivid and broader on ſome ſpecimens, than I had ſeen it before. Sol. of tin.

Sulphur with the ſhells produced all the colours, but then they were very tender throughout. I expoſed the ſhells a ſecond time in the fire for fifteen minutes more, without perceiving any difference. Sulphur.

One tea-ſpoonful of water, ſaturated with green vitriol, being put to about ſix or eight ounces of the ſolution of pot-aſh, I poured ſome of the clear liquor on to the ſhells, and expoſed them to the fire as before; and when they were examined Sol. of pot-aſh with vitriol.

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in the dark, no colours appeared, except a white light.

Sol. of vitriol. A ſolution of green vitriol only, gave no light. The vitriol having changed the colour of the ſhells, in appearance, to a reddiſh kind of iron ore, and this internally as well as externally.

Cor. ſub. Corroſive ſublimate with the ſhells produced the colours. The red and yellow were very bright. The green and blue were not ſo vivid.

Experiments upon acids.

AS I had entertained ſome doubts, whether theſe colours were actually produced by the ſeveral metals in their ſolutions, or by the acids themſelves, or that an union of both were abſolutely neceſſary to cauſe thoſe effects; and, for fear of being too precipitate in theſe matters, it was now the more proper to examine the effects of thoſe materials in a different way, by applying them ſeparately to other ſhells in the crucible. But before I begun the experiments with the metals, I was naturally drawn into a ſeries of experiments in conſequence of examining the aqua fortis alone, which was the firſt experiment I ſet about.

I poured an equal quantity of this acid, (as I had done before) upon another parcel of ſhells in a crucible, and then expoſed the crucible to the fire for the ſame time; as the heat was pretty nearly the ſame as in the former experiments. When the crucible was removed from the fire, and

and the ſhells were cold, I examined them ſeparately in the dark, after having ſhewn them to the light; and obſerved, that the priſmatic colours appeared, and ſomething broader than before, though not ſo vivid as with the ſolutions of ſilver, copper, and iron. The parts of the ſhells, that gave a white light, were conſiderably brighter than thoſe produced by the ſolutions.

With ſpirit of nitre, the ſhells gave a broad red light, ſomething like a burning coal, and brighter than the red above. The orange colour lay next to it, and at a little diſtance from that a blue, ſomewhat brighter than the orange. In two of the ſhells there was a greeniſh light, and the white parts were brighter than thoſe produced with aqua fortis. Sp. nitre.

From ſeeing that the colours were now exhibited upon the ſhells, in conſequence of the application of aqua fortis, or ſpirit of nitre, I had now a deſire to try the effects of the other acids upon the ſhells.

Half an ounce of oil of vitriol, with the ſhells, gave but a very *feeble* whitiſh light, many degrees darker than what the ſhells alone produce of themſelves by calcination: except in a few parts, and even thoſe were not to be called bright. The greater parts of the ſhells were totally dark; and when I ſcaled or broke them, the new ſurfaces, even in that caſe, gave only a very feeble light in general. Oil of Vitriol.

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Spirit of ſalt. An equal quantity of ſpirit of ſalt, treated in the ſame manner with the ſhells, ſhewed much the ſame appearances; but if there was any difference in the effects between the two acids it was in favour of ſpirit of ſalt.

Spirit of vinegar. Concentrated ſpirit of vinegar produced all the colours: the red and blue were the moſt vivid: but even thoſe were not ſo bright as what were produced by ſpirit of nitre.

Common vinegar gave the colours alſo: theſe however were fainter than the other.

By comparing the whole of theſe experiments together, and finding that oil of vitriol and ſpirit of ſalt give no colours to the ſhells; and that thoſe acids are conſidered by the chymiſts as having very little, if any, phlogiſton in them; I begun to entertain an idea that the principal cauſe why the oyſter-ſhells produce colours, depended upon a phlogiſtic or inflammable principle, and which the ſhells received from the metal and acids we have here produced.

To examine this matter further, I ſet about making other experiments; and begun the enquiry with obſerving the effects upon the ſhells, which the alcaline ſalts produced.

Experiments upon the alcaline ſalts.

Oil of tartar. The firſt I tried was oil of tartar. With this I produced all the colours. The red and green were the fainteſt; the blue was more bright, and occupied the greater part of the ſhells.

A ſmall

A ſmall quantity of pearl-aſhes, with the ſhells, gave the colours alſo, but not very vivid. Pearl-aſh.

Foſſil alcali produced a great quantity of very fine green, which was exceedingly bright: the blue was next in degree of ſplendor: the red and yellow were very faint. Some parts of the ſhells gave a very intenſe ſilvery white light, which continued to ſhine a full half hour. Foſſil alcali.

Spirit of ſal ammoniac gave an exceeding bright white light in every part of ſome of the ſhells: and in two or three ſmall parts there was an appearance of tender colours. Moſt of theſe ſhells continued to ſhine near half an hour. Spirit of ſal ammoniac.

Experiments upon neutral ſalts.

A quarter of an ounce of nitre applied to the ſhells in a crucible, as before, produced a very ſtrong red light, not unlike a burning coal; a little yellow, ſome green, and more blue; but the blue was brighter than the green. Nitre.

Borax refined gave very good colours, but not quite ſo vivid as nitre, excepting the red and blue. Some of the calx ſticking to one of the ſhells, I removed it, and then a good red appeared underneath. Borax.

Sedative ſalt, with the ſhells, produced the colours but faintly; excepting the red, and that was rather bright than otherwiſe. I uſed but a very ſmall quantity of ſedative ſalt; perhaps a larger quantity, and a proper degree of heat, would have made the colours more vivid. Sedative Salt.

Tartar of vitriol. Tartar of vitriol produced, on one ſhell, a very broad red light, and ſome blue on the confine of it. Other ſhells gave a white and blue light; and one ſhell gave an orange, yellow, blue and green colour; but thoſe appeared rather tender upon the whole, though very bright: and continued luminous for a conſiderable time.

I muſt not omit in this place, two or three experiments which I made upon other ſubſtances.

Swediſh acid. The Swediſh acid, or green fluor, put between the ſhells, produced the colours: the red was moſt brilliant; and thoſe parts that gave a white light were very vivid.

During the calcination of this greeniſh fluor, and before it fell into pieces, I fancied that there appeared upon it's ſurface a dull purpliſh light, of the phoſphoric kind: but, to be more certain, I ſhut the windows in the room (for the experiment was made about noon) and then I was able to obſerve the purpliſh light very diſtinctly: this light ſeemed to extend to ſome diſtance from the ſurfaces of the fluor.

Spar. The Derbyſhire ſpar, calcined with the ſhells, gave the colours alſo, and full as bright as the fluor deſcribed above.

Qeartz. A whitiſh ſpar, or quartz, from which I took a conſiderable piece of ſilver, gave the colours, but thoſe were not near ſo ſtrong as in the two laſt experiments.

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It was now time to make experiments upon the metals themſelves, and as they are ſuppoſed to contain phlogiſton I was the more ſolicitous to examine them.

Experiments upon metals.

Iron. The firſt experiment I made was with a piece of iron, about two inches long and one broad; which I laid between two oyſter-ſhells, and expoſed them in a crucible, to a moderate fire, for twenty minutes. In conſequence of this I obtained a red light; and what appeared to me the moſt remarkable was, that the red produced upon the ſhells was in thoſe parts only where the iron had been in contact with them. The other parts of the two ſurfaces were white. On breaking the ſhells the light was conſiderably brighter, and the red appeared to have penetrated almoſt through the whole thickneſs of the ſhells.

Iron wire. Round two large ſhells I wound two yards of iron wire. Theſe were then put into a crucible, with a ſoftiſh lump of the calcined ſhells, and continued in the fire for twenty minutes. On theſe ſhells I perceived in the dark a fine red and blue: there were other colours alſo, but then they were very faint. The ſoft lump, which was about the ſize of a large filbert, happened to lie at the bottom of the crucible: this gave a fine crimſon red on one part; on another, a very fine blue, three or four times larger than the red. In a remoter part

a little green appeared, and a feeble yellow in other places. On the back part of the ſame piece it was all red from end to end. I muſt here obſerve, that part of the wire which paſſed round the ſhells, was in contact with the ſoft lump; and the parts of the ſhells, at their edges, where thoſe wires had been more cloſely in contact, and had cut the ſhells next the bottom of the crucible, ſhewed the bright red and blue light mentioned above.

It may be proper to obſerve in this place, that moſt of the ſhells employed in the preceeding experiments were put into the crucible in a vertical poſition. And as they ſeemed to anſwer my purpoſe, I generally continued to obſerve the ſame method, though it is not always expreſſed.

Iron crucible. The laſt experiment was varied, by putting the ſhells into an iron crucible. The colours here produced were very diſtinct on ſome ſpecimens. The red and orange were next the iron: other parts gave a brilliant bluiſh white light, and ſome green. I then broke one of the ſhells, and obſerved that the red had penetrated one eighth of an inch deep, and a bright green appeared on the confine of it. Another ſhell which I had purpoſely laid horizontally at the bottom of the crucible, on being broken, gave an intenſe, ſilveriſh white light only. According to my uſual cuſtom of examining the ſhells, I ſeparated the firſt ſcale from it, and expoſed the new ſurface to the ſun, and was ſurpriſed to find the white light much more beautiful than before, and a little appearance of colours in one part of it.

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In the making of this experiment, I had previouſly laid a ſhell on the top of the vertical ones, in an horizontal poſition: but this afforded a very little light, and no colours.

I had frequently made the phoſphorus deſcribed by Mr. Canton in the Philoſophical Tranſactions, conſiſting of powdered ſhells and ſulphur, without having ever been able to obſerve any other phoſphoric colour than a fine yellowiſh white. However I had a mind to try, whether even in it's powdered ſtate, joined with a ſolid lump or two of ſhells, it would not afford colours. To this end, having mixed the powdered ſhells with flowers of ſulphur, in their proper proportions, I put half of the quantity into the iron crucible, and then laid upon it two pieces of ſhells, about half an inch in diameter, one upon the other horizontally. Upon thoſe I then put the other half of the mixture, and preſſed the whole down very cloſe. The iron veſſel was expoſed to a good fire for thirty minutes; and when it was cold I broke the maſs into two parts, where the ſhells had been put. On expoſing thoſe to the light, a breadth of tender colours appeared in the middle, above one inch in diameter, but they were not very bright: and they extended beyond the confines of the ſolid lumps. The red was in the centre, and was by much the broadeſt: a faint orange colour lay next to it; and a little yellow appeared beyond the orange. The other parts of the whole maſs ſhewed only a very tender, yellowiſh white light. Canton's phoſph.

To

Iron filings. To equal parts of iron-filings and nitre, I put ſeveral large fragments of ſhells: theſe materials were expoſed to the fire, and long before the crucible was taken out of the fire, the mixture had deflagrated. Theſe ſhells afterwards gave a faintiſh red, and other colours; but the white light was exceedingly bright. When I came to view the ſhells in the open day more attentively, I found that the calx of iron had formed a kind of cruſt upon them in ſeveral parts. This calx was carefully removed by a pen-knife, without ſcaling the ſhells: and then they were expoſed again to the light. Upon this, I perceived a very fine red in thoſe parts where the calx had been removed, as likewiſe a good green, and an intenſe blue very near it. That part of the ſhell where the calx was removed I obſerved to be firmer, and more compact, than any of the other parts.

This latter part of the experiment where the calx had been encruſted, turning out ſo ſucceſsfully, brings to my mind the firſt experiment I made with the ſolution of copper, and which produced the colours of the rainbow. I mention this circumſtance the rather, as the two experiments appear to illuſtrate each other. For in that experiment I had obſerved, with the aſſiſtance of a magnifying glaſs, that a larger quantity of the calx of copper lay next the red, than there appeared to be in any other part of the whole maſs.

Upon ſucceeding ſo well in the experiments with iron, it was ſuggeſted to me by Mr. Delaval, to

try

try what the difference would be with ſteel: on account of the greater quantity of phlogiſton it is known to contain.

Steel and Iron.

For this purpoſe I procured ſome watch-ſprings, and broke them into ſixteen pieces: each of thoſe were two inches long. I procured alſo an iron-plate of the ſame thickneſs with the watch-ſpring, or very near it. This plate was cut into four pieces two inches long alſo: and each breadth correſponded nearly with four of the ſprings, when they were laid cloſe and parallel to each other: for in this ſituation I diſpoſed them between the ſhells; I then put ſeven ſhells into a crucible vertically and cloſe to one another. On one ſide of the middle ſhell were placed four of the ſprings; and between each of the other ſhells and the crucible, on the ſame ſide, I put four more; making in the whole ſixteen pieces. On the other ſide of the middle ſhell I put one of the iron plates, and another plate between each of the ſhells and the crucible on that ſide, being in all four. In this ſituation the crucible was put in a clear ſea-coal fire for twenty minutes: and during the latter half of the time the heat was increaſed by blowing. Before each ſhell was examined in the dark, I took particular care to obſerve the direction in which the metals were expoſed to the ſhells. I began my obſervations with the middle ſhell: and preſented to the light that ſide of the ſhell firſt where the iron had been in contact. The colours ſhewed themſelves very diſtinct, but not ſo brilliant as I have ſometimes

ſeen

ſeen them in other experiments: the red, yellow, and blue, particularly. I then expoſed the other ſide of the ſame ſhell to the light, where the ſteel had been in contact, when the colours appeared conſiderably brighter; for in thoſe parts of the ſhell where the ſteel had touched, the red appeared very intenſe and bright; beyond that lay the other colours, but not in their order: for the green was next the red, then a little orange yellow; and beyond that, at a little diſtance, a very fine blue. I now examined the next ſhell, which had ſteel on both ſides: the colours in this were pretty nearly the ſame, eſpecially the red. The ſecond ſhell from the middle gave the ſame colours; but the red was leſs in quantity, and more intenſely bright, if poſſible, in that part of the ſhell which had been neareſt the bottom of the crucible. This difference was probably occaſioned by the pieces of ſteel having changed their ſituation by accident, becauſe on taking this ſhell out of the crucible, I found their ends lying one upon another towards the bottom. The third ſhell had the ſame colours on that ſide next the ſecond ſhell, and nearly as intenſe as thoſe upon the neighbouring ſurface, which the ſecond ſhell produced. But the other ſide of this ſhell which was next the crucible, where four pieces of ſteel had alſo been placed, and which continued in the ſituation I had laid them, gave an equal quantity of red light along with ſome other colours, as was obſerved in the firſt ſhell, reckoninng from the middle.

Having

Having finiſhed the obſervations where ſteel was employed, I then examined the other ſhells, where the plates of iron had been put. The firſt ſhell, next the middle one, as likewiſe the ſecond and third, exhibited the ſame colours with thoſe that the iron produced on one ſide of the middle ſhell; and their degrees of brightneſs were nearly the ſame, but much inferior to thoſe produced by the ſteel.

From this laſt experiment we collect, that the colours produced by the ſteel, being ſo much brighter and more intenſe than thoſe produced by the iron, the difference between them muſt have been occaſioned by the difference in the quantity of phlogiſton they reſpectively contained. Now ſteel, I underſtand, is always made, by giving to iron a greater quantity of phlogiſton than it contained before. And the method of attaining this end is generally by expoſing the iron for a long time in a charcoal fire: for charcoal, according to the doctrine of the chymiſts, abounds with phlogiſton. After that operation, nothing more is required to make ſteel, than to quench the iron in water. From theſe conſiderations it was very natural to try the charcoal itſelf, which proves ſo abundantly replete with phlogiſton, to iron.

Accordingly, between ſeveral ſhells, I put ſome broad pieces of charcoal: they were made flat on purpoſe, for the greater convenience of bringing their ſurfaces nearer to a contact with the ſhells. The whole was put into a crucible, and expoſed Charcoal.

to a ſeacoal fire for twenty minutes. Theſe ſhells afterwards produced exceeding fine colours. The red and blue were the moſt intenſe; next to them the green had the preference; and in two ſpecimens the colours were ſo finely intermixed, yet at the ſame time ſo diſtinct, that they reſembled, in ſome degree, the tail of a peacock, but very far beyond it in point of brilliancy.

The connection that Pruſſian blue is known to have with ſteel, from the manner of making it with iron and phlogiſton, encouraged me ſufficiently to ſee the effects of that colour upon the ſhells.

Pruſſian blue. A quantity of Pruſſian blue was therefore put into a crucible among ſeveral ſhells, and expoſed to the fire for twenty minutes. Theſe ſhells afterwards produced a yellowiſh phoſphoric white light, not very bright. From my former obſervations I had reaſon to apprehend, that the calx of the iron ſticking to the ſhells, obſtructed other appearances; I therefore ſcaled the ſhells, and then expoſed them again to the light: on doing this, I obſerved the red, yellow, and green, very diſtinct and bright: there was alſo a blue, but very faint, compared with the other colours. This blue, however, appeared brighter on removing one more of the laminæ.

Gold. It was now time to examine the other metals with the ſhells: and from conſidering the nature of gold, which ſo obſtinately reſiſts the moſt violent attacks of fire, without ſuffering the leaſt diminution

minution of it's weight, a ſuſpicion was entertained by ſome, that it would produce no colour when applied to the ſhells. But this theory did not correſpond with the experiment: for notwithſtanding I took particular care to have ſtandard gold from the refiner, and was very attentive in the management of it with the ſhells, the colours were produced by it. But then thoſe colours were not very bright, and rather tender upon the whole. The quantity of red equalled the ſize of the piece of gold, and that was nearly equal to the ſize of a guinea. It weighed about thirteen penny-weights; and was beaten flat for my purpoſe with an iron hammer. I mention this circumſtance, as I would have done any other, had it occurred, which might even cauſe a ſuſpicion of it's being the leaſt unfavourable to the experiment.

However, as I was not thoroughly ſatisfied with this laſt experiment, and being very ſolicitous to obtain every information in my power, I procured two ſmall parcels of *grain* gold from the refiner. One of thoſe parcels was put into a crucible; and whilſt it continued in fuſion, two ounces of powdered nitre was poured upon it; after the deflagration two ounces more were added; in a little time after, two ounces more. All this was done for the purpoſe of cleanſing the gold from any foreign mixture of other metal, &c. The gold before it was melted weighed three penny-weights. On taking the Grain gold.

gold out of the crucible, it weighed two grains leſs. This difference aroſe, perhaps in part, from ſcraping away the ſcoriæ with a piece of glaſs; for I perceived a few particles of gold to accompany it. The gold ſo prepared was put (without beating it flat) between two ſhells; and thoſe were then placed near the bottom of a crucible in an horizontal poſition. In another crucible, and between other ſhells, for there were ſeveral in it, I put the other parcel of *grain* gold, and which was nearly of the ſame weight with the former, and then placed the two crucibles in a wind furnace for ten minutes. The ſhells were afterwards examined in the dark, and within two hours after they were taken out of the fire; but, before this was done, it is proper to obſerve, that moſt of the ſhells from both crucibles had acquired a reddiſh purply hue, but particularly in thoſe parts where the gold had been in contact. On expoſing them to, and removing them from, the light ſeparately, I was greatly ſurpriſed at ſeeing not only all the colours on the ſhells from both crucibles, but at finding them far more vivid and beautiful than in the former experiment. The red however was by far the moſt predominant in the ſhells that were expoſed to the gold where no nitre had been put. For one of the ſhells was of a bright red all over, and on both ſides of it. Upon removing the upper ſcale, which I did with ſome difficulty on account of the firmneſs it had acquired, as likewiſe a ſecond,

and

and a third, the red appeared a little brighter; and, when I broke it, the ſame colour ſeemed to have penetrated quite through the ſhell. Half of another ſhell appeared red, and the other half green. Upon removing a ſcale, a very fine blue diſcovered itſelf under the green, and the red was more intenſe. A third ſhell ſhewed red on one ſide, and a kind of opal colour on the other; and a fourth was red in every part. One of the ſhells from the other crucible gave a fine red at one end, and next to it a yellow, then a great quantity of fine green, and a good blue beyond it. The other gave much the ſame appearances, except that it was more motled with the colours. In all thoſe ſhells there was no appearance of a white light whatſoever. The red in general was not ſo intenſe as I have met with in ſome of the preceding experiments. However, I think this red was very like the colour of the ſun when ſeen through a moderate fog in the ſpring or autumn.

As I found it rather inconvenient to procure grain gold, I had a mind to try whether a much leſs quantity would not anſwer the purpoſe as well. To this end, I covered ſome calcined ſhells with ſingle leaves of gold, and laid other ſhells upon them. Theſe were put into a crucible, and expoſed to a good fire for fifteen minutes. When they were cold, I obſerved the pieces of the leaves of gold upon different ſhells were a little changed in their colour: theſe I bruſhed off; and, after expoſing the ſhells to the light, the

Leaf gold

the colours appeared, but exceeding faint; the yellow was broadeſt and brighteſt, the green next, and the red the leaſt. Perhaps, if, inſtead of the leaf, I had put four or five leaves upon one another, they might have anſwered the purpoſe better.

Quick ſilver. An ounce of quickſilver was put to three ſhells in a crucible, and expoſed to a moderate fire about eight minutes: whether the ſhells were continued too long, or not long enough, I cannot tell: but on examining them in the dark, I only obſerved a very fine blue in different parts of them, and thoſe parts were the moſt remote from the bottom of the crucible. The breaking or ſcaling of the ſhells made no difference in the appearances.

Not being thoroughly ſatisfied with this laſt experiment, I repeated it again two months afterwards, but with a quarter of an ounce of quickſilver, and with the ſame number of ſhells. The crucible was now put into the fire with the ſhells in it, and when they were red hot, I dropped the quickſilver into the crucible, and continued the heat for three minutes more. After this I obſerved the colours in two of the ſhells very diſtinct and bright; particularly the red, yellow, and blue. The other ſhell gave a little red, ſome green, and a great deal of white light.

Lead. Lead produced the colours, but the red, orange, and yellow, were the brighteſt.

In

In another experiment, but with a leſs quantity of lead, the colours appeared more vivid, particularly the green and blue.

Thirteen penny-weights of the fineſt ſilver I could procure was put between two ſhells, and expoſed in a wind furnace for eight or ten minutes, by which they acquired a dirty purpliſh hue, and a firmer texture in ſome parts. In the ſame ſtate they were expoſed to the ſun: but no kind of light appeared upon thoſe purpliſh parts. I then removed the purpliſh hue by ſcaling the ſhells with a penknife as before: in this ſtate they were expoſed again to the light; the colours now appeared to great advantage. The red and blue were the moſt intenſe and beautiful; the green next, and the yellow and purple were rather faint. Other parts of the ſhells were white, and exceedingly brilliant: thoſe continued luminous a full half hour. I do not mean by this, that the colours laſted ſo long, but only that ſome of the whitiſh light was perceptible at the end of half an hour. Silver.

I repeated the laſt experiment with about five penny-weights of ſilver, in grains, from the refiner: theſe were put among *ſeveral* ſhells in a crucible. And after the uſual treatment of ſcaling, &c. one of them produced a fine red and blue only; another a little red, with a great quantity of blue, and a little purple; a third gave a green and blue, with a conſiderable quantity of a ſilvery, intenſe, white light; and a fourth ſhewed a little ſtrong red, yellow Grain ſilver.

yellow and blue. Two other pieces afforded a white light, but intenſely bright.

Native ſilver. A piece of native ſilver, from the collection of Guſtavus Brander, Eſq; who favoured me with it, produced the colours: but then they were not near ſo beautiful, as thoſe which appeared in the two laſt experiments.

Common copper. Two ſmall plates of common copper were applied to the ſhells, and after being expoſed in the fire for fifteen minutes, the ſhells gave the colours; but not ſo finely, as in the *firſt* experiment, where the ſolution of the ſame metal was employed.

Very fine copper. A very fine ſpecimen of copper, from the ſame collection mentioned above, produced the colours nearly the ſame with the laſt experiment; except that the red and yellow were conſiderably brighter and more intenſe.

In the courſe of thoſe laſt experiments where colours have been produced by the preſence of metal, different degrees of heat were employed: I was obliged therefore to proportion the time of continuing the crucibles in the fire accordingly; but not without giving ſome attention to the nature of the different materials employed in each experiment. Nevertheleſs, even though I obſerved theſe precautions, I would not be underſtood that I am at all acquainted with the degree of heat that is neceſſary for producing the colours to the greateſt advantage.

Tin. With Tin, I obſerved, the experiment ſucceeded pretty well, the red and yellow were but faint, compared

compared with the green and blue: thoſe being very bright.

I was now deſirous of ſeeing the effects upon the ſhells from one of the ſemi-metals, which is ſaid to contain a great deal of phlogiſton: what I mean is zinc. I had no ſooner gone through my uſual proceſs with it, but I examined the ſhells, even before they were quite cold, without being able to diſcover any colour but a little white light. I ſucceeded no better when they were cold. The outward appearance of the ſhells in the open day, was a little yellowiſh; and from what I had obſerved before, it ſeemed very neceſſary to remove this yellowiſh tinge: I therefore ſcaled the ſhells, and examined them afreſh, when there appeared very large breadths of fine priſmatic colours on every ſhell: but the red was by far the moſt intenſe and bright. I then broke every ſhell into ſeveral pieces: and, on examining them ſeparately, after their being expoſed to the light, there appeared in many parts all the colours; and in others a moſt beautiful intenſe ſilvery white light, which continued to ſhine a full half hour. Zinc.

I now thought it time to put an end to thoſe kind of experiments, notwithſtanding there are many other ſubſtances that I have not tried, which might have been brought into this claſs with propriety: ſuch as reg. of cobalt, biſmuth, marcaſites, cinnabar, lapis lazuli, &c. but theſe I conſidered as being unneceſſary, from having ſucceeded ſo effectually with the metals themſelves, as well as with that

very inflammable metalline ſubſtance deſcribed in the laſt experiment.

In the courſe of the preceding experiments I frequently fancied it was neceſſary to clean, and ſcrape, the ſhells very well before they were put into the crucible; having obſerved that a few of them, ſo treated, ſeemed to produce the colours more lively. Whether it was really the caſe or not, I am ſtill ignorant: however, in my future experiments, theſe precautions were taken.

And here I beg leave to obſerve, that where I hereafter mention, in any of the following experiments, reſin, or ſugar, or glue, &c. produce the colours, I wiſh to be underſtood, that thoſe ſubſtances were reſpectively put into a *crucible* along with the ſhells. This deſcriptive part was left out merely to avoid repetitions.

I HAVE now completed the experiments upon phoſphori, and the priſmatic colours produced by the ſolutions of metals, by ſome of the acids, (for the other acids did not produce the colours) and by all the alcalies; as well as by ſeveral of the neutral ſalts: and having extended my reſearches to the metals themſelves ſo ſucceſsfully, where no other ſubſtance was introduced, but the common baſis I made uſe of, namely, the calcareous earth: and finding that, among other ſubſtances, ſteel, as alſo charcoal, and Pruſſian blue, which are allowed to contain a conſiderable ſhare of phlogiſton, produce the priſmatic colours in an eminent degree;

there

there appears to me a further foundation for entertaining more than a conjecture, that the priſmatic colours, as well as the light in general of all phoſphori, depend in a great meaſure, if not entirely, upon that inflammable principle which the ſhells received, in conſequence of it's being diſengaged by the force of fire, from the bodies they were in contact with in the crucible.

Nothing more remains, therefore, to eſtabliſh the principle and make it general, than to examine the effects of other ſubſtances upon the ſhells, which are known to abound with phlogiſton. And if ſimilar appearances are to be obtained from them, we may then ſafely pronounce the principle general: at leaſt, till exceptions are brought againſt it.

Experiments upon INFLAMMABLE BODIES.

ONE tea-ſpoonful of linſeed oil put to the ſhells in a crucible, and expoſed to the fire for twenty minutes, gave a feeble white light, and a very intenſe blue. In ſome ſpecimens a little orange and green, but no red. From the reſult of the ſecond experiment with quickſilver, I think that theſe ſhells were not continued long enough in the fire. Linſeed Oil.

Olive oil gave a good blue and green, and a faint orange colour. Olive Oil.

Oil of turpentine produced a fine red and blue. Some other parts were green and white, but thoſe were much fainter. Oil of turpentine.

Wax. A lump of wax, the ſize of a very ſmall nut, gave the colours alſo; and rather bright than otherwiſe.

Reſin. Reſin produced the red, yellow, green, and blue, better than wax.

Pitch. Pitch, gave the colours much the ſame, except that the blue was brighter than the reſt.

Gum arabic. Gum arabic gave a very intenſe bright red and blue, with a little yellow, and a dirty browniſh hue in other parts.

Copal. Copal produced the colours, but not near ſo bright as gum arabic.

Tragacanth. Gum tragacanth afforded colours alſo; the red exceeded in brightneſs the red produced by copal, the yellow and blue appeared rather better than the green.

Flame. Flame from a large wax candle, and alſo from ſpirit of wine, produced very fine colours; the red and yellow were the brighteſt, the green and blue next; the violet was leſs vivid.

Theſe colours were not only produced by burning the ſhells in a crucible after they had been expoſed to the different flames for two or three minutes, but alſo without expoſing the ſhells to any other heat than the flame itſelf, it being fully ſufficient for the purpoſe. In theſe experiments I obſerved that the colours, produced by the flame of ſpirit of wine, were rather weaker than from the flame produced by wax.

Soot. A table-ſpoonful of lamp-black, or ſoot, put into the crucible, and expoſed to the fire for fifteen

fifteen minutes, gave very fine colours alſo; the yellow, green, and blue were the moſt brilliant, the red next, but very broad; and the purple fainter.

I took a quarter of a ſheet of white paper, and cut it into ſeveral pieces; theſe were put into a crucible along with ſeveral ſhells. I then continued it in a fire for twenty minutes. On examining thoſe afterwards in the dark, I obſerved that moſt of the ſhells had acquired the colours, and thoſe were rather brilliant than otherwiſe. Two of them, however, ſhewed only a fine blue light. Theſe I expoſed again in the fire for ten minutes more (but in the crucible:) in conſequence of this, they produced all the colours; but the blue was weakened conſiderably. From the effects produced by this ſecond application of heat, I ſuſpect that the ſhells in ſome of the preceding experiments, which only afforded green or blue, were not continued long enough in the fire. Paper.

Linen, treated in the ſame manner, (except that I continued it in the fire for thirty minutes) gave the colours rather ſtronger than the paper. Linen.

I now put a little cotton along with ſome ſhells, and after theſe had been in the fire for fifteen minutes, I found that two of the ſhells exhibited three of the colours very vividly, viz. red, green, and blue; the other ſhells gave a blue and white light only. Cotton.

A lump of ſugar, the ſize of a hazle nut, expoſed with the ſhells, in a crucible, to a fire for twenty Sugar.

twenty minutes, produced ſome exceeding fine ſpecimens of colours. In three of the ſhells there were great quantities of fine red and green, a little yellow, and an intenſe blue, but leſs purple. In one of the ſhells the colours lay exactly in their order.

Wheat flour. Flour of wheat (about three tea ſpoonfuls) put into a crucible with the ſhells, produced the colours; but then they were rather faint, excepting the red and blue, and thoſe were a little brighter than the reſt.

Flour with ſolution of pot-aſh. Some of the ſame flour, with a ſolution of pot-aſh made into a cake, and toaſted a little, gave all the colours without the application of the ſhells; but the red was a heavy dirty colour. This was not the caſe with the other colours, they being rather vivid, and the parts that gave a white light were very bright.

Charcoal, &c. Oil of vitriol with the ſhells producing no colours, and ſcarce any light; I put ſome powdered charcoal to them, and then expoſed the ſhells again to the fire for fifteen minutes. After this I obſerved two or three of the colours, but they were rather faint. However, the whitiſh appearance was conſiderably improved.

Spirit of ſalt with the ſhells producing no colour, I put ſome powdered charcoal to thoſe alſo: in conſequence of which, the colours appeared, and nearly of the ſame ſtrength as in the laſt experiment.

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I had an inclination to vary the experiment with oil of vitriol, by mixing ſome powdered charcoal with it, and making a kind of paſte thereof. This paſte was put into a crucible, and I then forced the ſhells into the paſte. The crucible was in the fire about fifteen minutes. By this treatment the colours were improved, and the blue particularly; it being very fine. Other parts gave a very bright white light.

Tartar of vitriol with charcoal gave a fine white light to the ſhells, and a faint appearance of red and yellow. Tart. vit.

Spirit of wine afforded only a bright greeniſh and bluiſh light. I made but one trial with it. Perhaps a different quantity of it, and a different heat, would have produced more colours. Sp. wine.

A quarter of an ounce of animal fat with the ſhells exhibited a fine red, and blue, on two ſhells; and a purpliſh ſilvery white light all over the other parts. The other ſhells were exceedingly bright, and of a ſilvery hue. Fat.

Freſh butter produced the colours, and rather vivid than otherwiſe; but the blue was the brighteſt: and the parts that were white, appeared not quite ſo intenſe as thoſe in the laſt experiment. Butter.

A piece of glue, the ſize of a ſhilling, broke into ſmall pieces, and put between the ſhells, produced one very fine brilliant ſpecimen of colours, without ſcaling any part of the ſhell. The red was rather brighter than what nitre produces; Glue.

and

and this colour was at that end of the ſhell which was neareſt the bottom of the crucible. The yellow lay next to it, then the green; but it was much broader, and more intenſely bright. The blue next, and a faint purple appeared at the top of the ſhell. I now removed the upper ſcale of this ſhell, and expoſed it again to the light. On doing this, two ſeparate reds appeared where one only had been viſible before; but theſe were conſiderably brighter: and what ſeemed the moſt curious to me, was an exceeding fine brilliant *purple light*, which lay in the interval between the two reds. The yellow, green, and blue, were a little improved: but the purple at the top of the ſhell was totally vaniſhed. In this very fine ſpecimen there was no appearance of any white whatſoever. There were three more ſhells in the ſame crucible at the ſame time; but not one of them afforded any colours that were ſo remarkably fine as the ſpecimen I have now been deſcribing.

Wool. I put ſome wool between two ſhells, and after expoſing thoſe in a crucible to the fire for ten minutes, and then to the light, I perceived a very fine red, green, and blue light in one of the ſhells, and only a blue one on the other.

Skuttle-fiſh. The bone of the ſkuttle-fiſh, which had been thrown by the ſea upon the beach, and which contains a conſiderable ſhare of oil, even in it's dried ſtate, was put into a common ſeacoal fire.

In

In a very little time it was calcined in ſome parts, and not in others; but being of an exceeding looſe texture after the calcination, it was with ſome difficulty I could preſerve three or four pieces from breaking into ſmaller parts. Theſe were expoſed to the light: and when I examined them in the dark, there was a very fine red and blue light on thoſe parts which were rendered white by the calcination: the other parts, that were not calcined, had a black appearance, and afforded no colours.

Hair, burnt with the ſhells, produced a red and blue light; thoſe colours were rather bright than otherwiſe. Hair.

Amber being exceedingly inflammable, I was not a little ſolicitous to try it's effects upon the ſhells. I procured therefore three flat pieces of amber about the ſize of a ſhilling, and near the thickneſs of a crown piece. Theſe were put between four ſhells in a crucible. After expoſing them to a good fire for twelve minutes, I took an opportunity, when the ſun was ſhining, for it was in December, to expoſe each ſhell to it. I was exceedingly ſurpriſed at ſeeing the very fine colours they exhibited. The orange and yellow predominated in a moſt brilliant manner upon one ſhell; the red and green were deeper and more intenſe upon another. One of the middle ſhells gave all the colours exactly in their order, and were very ſplendid. The red part of the ſhells, in all theſe ſpecimens had been next the bottom Amber.

of the crucible; and what proved rather a rarity to me was the purple colour, which appeared full as bright as that which the glue produced. None of theſe colours appeared ſo vivid as when the upper ſcale was removed. But it muſt be remembered, that this is not always practicable, on account of their acquired hardneſs; and yet ſcraping of the ſhells does not anſwer ſo well, at leaſt with me. In this caſe, I have recourſe to breaking them into ſeveral parts; by which means I have an opportunity of examining the edges, after they have been expoſed to the light, and ſeeing how deep the different colours penetrate the ſhells; and which, in many inſtances, I have obſerved to be luminous throughout.

Solid phoſph.

Not being able to recollect any other ſubſtances of this kind, I was curious to try the effects of the moſt inflammable artificial ſubſtance I was acquainted with. This was the phoſphorus of Kunckel.

A ſmall piece of this ſolid phoſporus, about the ſize of a very ſmall pea, I put between two ſhells that were placed horizontally, in a crucible, and expoſed them to a moderate fire, when a very fine bright flame appeared between the ſhells. In five or ſix minutes this flame, produced by the phoſphorus, expired. I continued the crucible in the fire for three or four minutes longer: and then examined the ſhells after they had been expoſed to the ſun. The ſplendor of the light in general was very great. The blue appeared the brighteſt, and the green and yellow

yellow were rather faint, but brilliant. Upon another ſhell there was a tender orange colour and green, very beautiful; beſides a faint appearance of the other colours, excepting red. Theſe ſhells continued to ſhine above thirty minutes.

From the many obſervations I had made upon theſe colours, during ſuch a variety of experiments, I had very great encouragement to try whether the yellow, green, and blue colours, in ſome of my beſt ſpecimens, might not be *changed* to red, by bringing the inflammable ſubſtances into contact with thoſe parts that gave ſuch colours. Changing of colours

Accordingly I put ſeveral of thoſe fine ſpecimens again into the fire, in five different crucibles: after having marked the places on the ſhells (by ſcratching them) which gave thoſe colours only. To theſe I applied the inflammable ſubſtances, ſuch as paper, ſugar, gum arabic, amber, and charcoal, ſeparately. And though I found it exceedingly difficult to bring moſt of theſe inflammable bodies into contact with all the parts of the ſhells that gave the yellow, green, and blue colours, on account of their irregular ſurfaces; yet with a little induſtry theſe difficulties were in a conſiderable degree removed. For, after expoſing the ſeveral crucibles to the fire, but at different times, and then examining the ſhells in the dark ſeparately, I obſerved that many of the colours were entirely vaniſhed; and in their room the red had taken place; and particularly in three ſpecimens, where very broad and intenſe blues had predominated,

the greateſt parts, in general, of that colour upon the three ſhells were, by this treatment, changed to a very fine red.

I likewiſe obſerved that the ſhells in general were more hard and firm, after this ſecond proceſs, than before; I mean in thoſe parts where the new reds were produced in the different ſhells.

WE have not ſcrupled to ſet down all theſe trials in the five crucibles as one experiment, becauſe I was apprehenſive that the mentioning of them ſeparately might be thought too tedious.

Light.

I took one of the ſhells, which gave no other phoſphoric light than what was cauſed by calcination, and expoſed it for ten minutes to the rays of the ſun, collected by a mirror which was about fourteen inches diameter; and then removed it into the dark. On that part of the ſhell where the rays were thrown, the light appeared conſiderably brighter, but without exhibiting any colours. It did not laſt long: neither was the brightneſs of the light to be reſtored by expoſing the ſhell in my uſual way (through the curtain) to the open day.

I now expoſed one of the fine ſpecimens of the ſhells to the collected rays, as before, for two or three minutes; and by moving the ſhell a little, the greater part of it's ſurface was illumined by thoſe rays. This ſhell, in the dark, ſhewed the priſmatic colours very vivid and beautiful, and I think rather more ſo than I had ever ſeen them on any of my former ſpecimens.

Had

Had a proper opportunity offered I ſhould have calcined ſome ſhells with light congregated by a burning-glaſs, on purpoſe to have tried whether they would have exhibited phoſphoric appearances, and particularly the priſmatic colours; becauſe it is not impoſſible but that ſuch an experiment may very likely produce ſome information of conſequence in theſe reſearches with reſpect to the inflammable principle.

WE muſt here obſerve, that moſt of the preceding experiments were made in the winter, and in all ſorts of weather. However, I would not be underſtood to mean by this, that thick fogs, and heavy rains, are ſo advantageous for ſeeing the colours, as a clear ſun, or even a cloudy day: and yet I have been able to obſerve the priſmatic colours pretty diſtinct, even in thoſe unfavourable ſeaſons.

I HAVE now gone through all the experiments upon inflammable bodies that I apprehend are moſt eſſential for eſtabliſhing the principle upon which the priſmatic colours in the ſhells, appear to depend. As to the nature of this principle, whether it be perfectly ſimple, or whether it is combined with any other matter, I profeſs myſelf totally ignorant. It is enough for me to have diſcovered it's effects, which are ſo neceſſary for exhibiting the priſmatic colours, by a ſeries of experiments in the vegetable, animal, and mineral kingdoms.

I ſhall

I SHALL now make a few obſervations upon the ſhells themſelves, which are found to receive this ſubtile inflammable matter.

Calcareous ſhells, from the weak coheſion of their parts, are eaſily broken, or ſeparated, into numberleſs ſcales: and even thoſe again are found capable of being further ſeparated and reduced, ſo as to become inviſible to the naked eye.

Now I find that the coheſion of thoſe parts may be conſiderably increaſed, in many caſes, by only adding a quantity of the inflammable matter: for in the parts where the red appears, a firmneſs and compactneſs frequently takes place in conſequence of it, which I did not obſerve in others where no red appeared.

And as I find that the red appearance may be always obtained in any part of the ſhell by only bringing the part required to have that colour into contact with the ſubſtance that gives the phlogiſton; we have a proof that there is no particular diſpoſition in the ſhell to produce that colour only, without the aid and aſſiſtance of the inflammable principle.

By this red happening in conſequence of the contact and the ſtronger coheſion of the parts in that place, it appears, that more phlogiſton muſt have penetrated the ſhell in the part where the red was produced than in any other remote from it. This colour therefore depended upon the quantity of phlogiſton received in that particular part.

And

And this reaſoning received the fulleſt confirmation from the experiment which I made, where the other colours were ſeverally changed to red by putting the ſeveral parts of the ſhell which produced them into contact with the ſubſtance that imparted the phlogiſton *.

From all theſe obſervations we may now collect the cauſe why thoſe parts which give the red appearance acquire that degree of denſity, which appears ſo eſſentially neceſſary for occaſioning that particular colour: and that particular degrees of denſity are eſſentially neceſſary to exhibit colours, appears, from obſerving that the colours in any of the ſhells are not of a very laſting nature: for three or four days time make conſiderable variations in thoſe colours, by the ſhells imbibing moiſture from the air; which muſt make an alteration in their component parts, and conſequently vary their denſities: which is the thing we intended to prove.

IN regard to the ſhells giving the priſmatic colours in the beautiful manner they do, when viewed in the dark; and yet affording no other appearance in the open day, than the ſame white light they acquired by calcination; a very little conſideration may, perhaps, explain this matter, as we find it ſo exactly conſonant with other phœnomena.

The ſtars, and even the moon itſelf, are totally obſcured when the ſun is in the meridian, by the flood

* Page 59.

flood of light that is diffuſed over the whole of our atmoſphere. And yet when that ſuperior light is withdrawn, how very ſplendid does the moon appear! Now the darkneſs we produce by art, exceeds this natural one by many degrees. No wonder then, that the difference between the external light, and the cloſet we have deſcribed, is far greater than that between noon and midnight; and more eſpecially whilſt the moon continues ſhining. It is therefore entirely owing to this very great difference, that the priſmatic colours in the ſhells become ſo viſible in the dark, when the eye is properly prepared. So likewiſe, thoſe colours, though they actually continue to exiſt in the open day, diſappear immediately on being expoſed to the ſun, by the very great abundance of reflections and refractions of light with which they are ſurrounded.

And however ſtrange it may appear, I have ſome foundation for apprehending, that the ſplendor of the priſmatic colours in the open day, could we perceive them, is far greater than that we obſerve in the dark. This notion ariſes from many obſervations that were made during the whole courſe of the preceding experiments. For when the different phoſphori were brought from the light into the dark, the ſplendor of the colours was exceedingly more vivid and beautiful in the firſt inſtant, than they appeared even in the third of a ſecond afterwards. The diſappearing of this great degree of brilliancy was ſo ſudden,

that

that I could ſeldom move the ſubſtances, which exhibited it, a few inches from the curtains, before a conſiderable alteration in it's vividneſs had taken place: notwithſtanding the time in moving the phoſphori from the curtains to the convenient diſtance for obſerving, (it being about 12 or 15 inches) was not more than half a ſecond.

IN deſcribing the preceding experiments, I have ſet down the ſeveral colours which were obſerved in each ſhell; the order in which they happened to lie; and generally their degrees of brightneſs, as they appeared at 12 or 15 inches from the curtains. Nevertheleſs, I ſhall not be ſurpriſed, if another obſerver was to find the colours, in all theſe reſpects, conſiderably different, even at that diſtance, on account of a variety of circumſtances which accompany each experiment; and which may be materially neceſſary in producing the colours, as I found them. Such as the quantity of matter, or materials, that is, or are, to part with the phlogiſton; the manner and diſtance of their application to the ſhells; the uneven and irregular ſurfaces of thoſe ſhells in general; and the great uncertainty of the heat employed in each experiment, &c. Theſe are conſiderations of ſome moment with me, and, in my opinion, render it very difficult, nay, perhaps, impoſſible, to make any two experiments with the ſame materials, ſucceed in every reſpect alike: and as I had no exact rule to guide me in the

great number of experiments I have made, I ſhall not wonder if more able obſervers ſhould produce the colours in far greater perfection than I have done: for I am ſo young in theſe reſearches, that I do not know whether I have yet ſeen any one of the priſmatic colours in the greateſt perfection poſſible. However, notwithſtanding all the conſiderations and difficulties I have mentioned above, no one ought to be diſcouraged from making the experiments, as he will certainly obtain the priſmatic colours in ſome degree or other, and ſometimes in their order; even if his attempts ſhould happen to be made rather careleſsly than otherwiſe.

And to ſhew how eaſy a thing it is to produce the priſmatic colours, I will give an example, where nothing more is required than a few common oyſter-ſhells, and a good fire of any kind. For if thoſe ſhells are thrown careleſsly into the middle of it, and continued there for a proper time, which may be from ten minutes, a quarter, half, or three quarters, of an hour, to one, two or three hours, according to the thickneſs of the ſhells, and the degree of fire they are expoſed to, the ſeveral ſhells, after being cold, will give the priſmatic colours in a beautiful manner, when they are removed from the ſun into the dark ſuddenly; and the eyes have been previouſly prepared a little to receive them.

My ſucceſs of late has been ſo very great in this reſpect,

reſpect, that I ought not to omit giving a general account of it.

Half a dozen of thoſe common ſhells I forced into the middle of a ſea-coal fire, and continued them there one hour. Upon taking them out, the ſurfaces appeared to have a dirty yellowiſh hue. When they were ſcaled and expoſed to the light, the colours in the dark exceeded every thing I had met with before. In one, the green predominated in ſo extraordinary a manner, that I thought it was the brighteſt and moſt beautiful colour I had ever beheld. In another the yellow and red were very broad, and moſt intenſely bright. In a third, all the colours were exhibited to very great advantage. The fourth ſhewed a very fine red and blue, but the yellow and green fainter. The fifth and ſixth exhibited very fine colours alſo that were exceſſive bright. Theſe colours did not only excel in their ſplendor of light, but in their continuance; for I was able to obſerve them near three minutes. In all theſe ſpecimens there was no inſtance where the colours, upon the whole, lay exactly in their order.

Upon expoſing ſome of thoſe ſhells to the fire again for fifteen or twenty minutes more, the colours were varied in many places, and new ones appeared in others.

From this laſt experiment, and ſome former obſervations, I am induced to ſuſpect, that the ſhells, during their calcination in the fire, are continually undergoing ſuch a variety of changes

in their component parts, by the inflammable principle they receive, that they are almoſt every moment differently diſpoſed, in different parts, to exhibit different colours. It is therefore probable, that there is a time for taking the ſhells out of the fire, when they are capable of exhibiting the colours in the moſt favourable manner; but to ſeize upon that moment, when the ſhells are diſpoſed to give the colours to the greateſt advantage, is a circumſtance beyond the reach of my abilities: and indeed, to ſay the truth, I have always found that chance did more for me in this reſpect than judgment; as for example. At one time I have obſerved the green predominate over the other colours in an extraordinary manner as to breadth and brilliancy; at another, the blue excelled in an eminent degree; and ſo on with reſpect to the other colours, excepting the purple, which I very rarely was able to obſerve, there being but five or ſix inſtances where that colour appeared very bright. However, I found upon the whole ſuch an endleſs variety of the moſt beautiful colours to be produced in this manner by fire upon the ſhells, that it hath amply repaid me for all the trouble I have had in theſe curious reſearches.

Candle-light. To ſhew how excellent ſome of theſe ſpecimens (which I frequently obtained) were as phoſphori, the light of a candle, at eight or ten feet diſtance, was ſufficient to make them ſhine a little in the dark; and this light would continue for

for ſome time, but without giving any of the colours.

In treating of the priſmatic colours exhibited by gold, I mentioned the effects which a ſingle leaf of that metal produced: and having obſerved that the red appearances are occaſioned by the contact of metal, or other ſubſtances, which part with the phlogiſton to the ſhells, and the great difficulty there is to bring a large ſurface of metal into contact with thoſe calcareous ſubſtances, which are generally very uneven and irregular on their ſurfaces, I had a deſire of trying whether the whole ſhell might not be made red by laying a number of ſuch leaves one upon another all over the ſhell. Upon making the experiment with ſix leaves of gold I was not diſappointed. For theſe leaves were made to lie pretty cloſe to the ſhell by blowing upon them gently. In this ſituation the ſhell was put horizontally into a crucible, and upon it I laid another ſhell. Theſe were expoſed to the fire for ten or twelve minutes, and, after expoſing them to the light, the whole of the ſhell where the gold had been in contact, exhibited a red light that was rather brilliant than otherwiſe, excepting a very ſmall part which had a faint greeniſh hue. The upper ſhell exhibited a red light alſo, but this red appearance was ſomething weaker, and only obſervable in two or three parts on that ſurface which lay next the gold. Leaf gold.

After trying a few experiments upon ſome ſmall pieces of mother-of-pearl, I had encouragement Mother of pearl.

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to calcine a whole ſhell of that kind. It was very large, and required ſome care in the calcination to prevent it's breaking, and ſcaling, into ſmall parts, which it is extremely ſubject to, by that treatment. This ſhell I put into a moderate fire, and continued it there a little more than twelve minutes. But, notwithſtanding all my care on taking it out of the fire, the ſhell broke into two pieces, and many ſcales ſeparated from it. Thoſe pieces I expoſed to the light at different times; upon obſerving them in the dark, the colours on both ſides of thoſe pieces exhibited the colours very finely. The red, yellow, and blue appeared intenſely bright and beautiful all over both fragments, except a little green, which was rather fainter, and a few ſmall parts that gave no colour at all. There was no appearance of any white light whatſoever on either piece. Upon the whole, I do not think that the colours were finer than what the oyſter-ſhells produce: but the magnitude of theſe pieces, and the quantity of colours they exhibited, were ſuch favourable circumſtances as might prejudice a common obſerver to give them the preference.

Beautiful as thoſe colours were, I do not recommend mother-of-pearl before the oyſter-ſhells for making experiments with, on account of their being ſo eaſily broke, and ſeparated by calcination. Beſides they are attended with a conſiderable expence, if great quantities are made uſe of. On the other hand, oyſter-ſhells are eaſy to be had, and

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even without money, which to ſome philoſophers is no inconſiderable argument in their favour.

In mentioning one object of œconomy, it brings to my mind another that may ſerve as a ſubſtitute to make thoſe kind of experiments in, which will ſave expence, and anſwer, in ſome degree, many of the purpoſes of the dark cloſet; what I mean is a kind of box or *camera*, which muſt be ſecured properly in every part with black cloth to keep out the light effectually. This box ſhould have a drawer in it, which muſt be ſo contrived as to keep the light out alſo when it is ſhut; and the drawer muſt be large enough to hold the ſhells or other ſubſtances that are to be examined in the dark. And that there may be no difficulty in breathing the air within the box, two curving tubes may be inſerted ſo as to communicate with the external air: one of thoſe ought to be at the bottom, and the other at the top, of the box. The obſerver himſelf, whilſt he is in this darkneſs, may, by a little contrivance, very eaſily puſh the drawer out to the light, and bring it in again as readily, without any inconvenience. This apparatus ought to be in the open air, and if in the light of the ſun ſo much the better.

DURING the making of all the preceding experiments, I took every opportunity in my power to learn whether the priſmatic colours, in phoſphoric ſubſtances, had been diſcovered by any of the learned who have written upon the ſubject of phoſphori. Hitherto I have not been able to make

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out any more than that a *red* light has been obſerved in two or three different phoſphori, and particularly in one by the celebrated Marggraaf, which is mentioned in a *memoir* of his printed in the Berlin acts for 1750, vol. vi. p. 156.

" La ſolution d'écailles d'huitre faite dans l'eſprit " de nitre, précipitée par l'eſprit de vitriol, & cal- " cinée avec les charbons, donnera une lumiere " rouge."

When I firſt read the account of this experiment, it appeared ſomewhat ſingular to me, that the phoſphoric preparation therein deſcribed gave only a red colour in the dark, notwithſtanding it had been expoſed to a charcoal fire: but, upon a little reflection, I have ſince ſuſpected, that the heat M. Marggraaf employed was either too violent, or too long continued; for I have frequently obſerved, that the colours, which the ſhells exhibit, may be entirely deſtroyed in both theſe caſes; nay, I even ſuſpect, that thepurple colour is generally occaſioned by a *leſs* heat than any of the other colours.

Had I met with this curious experiment of M. Marggraaf's before I obſerved the priſmatic colours at firſt, it is not improbable but that this undertaking of mine would have been ſhortened conſiderably, by falling upon a different train of experiments. However, ſince the method I have taken hath been productive of ſome material information, it may, upon the whole, perhaps, be as well as it is; for in theſe curious reſearches, where a ſeries of experiments were my guide, many circumſtances conſpired to throw more light upon

upon the ſubject than I might probably have found by proceeding upon a different plan.

In conſequence of my ſteadily purſuing thoſe experiments, ſtep by ſtep, with great application and induſtry, and obſerving every circumſtance in each experiment with all the care and attention I was able, in hopes of tracing out ſome ſecret principle upon which the phoſphoric power depended, *chance* threw into my way ſo uncommon an object, that I conſidered it as a treaſure in experimental philoſophy to exerciſe my moderate talents upon. No day paſſed, from that time to the completion of theſe obſervations, without making ſome new experiments, or enlarging my ideas; and, after having extended my reſearches ſo far as to be able, at all times, by the ſimpleſt means, to produce the priſmatic colours ſo very intenſe and beautiful, without ever failing in my attempts; it now appears a matter of very great ſurprize, how I avoided ſtumbling upon the diſcovery immediately after I had obſerved the effects upon the ſhells that were calcined in the cracked crucibles mentioned before in an early part of this work. What a deal of fatigue and trouble would the putting of an oyſter-ſhell into the fire have prevented! Who would have thought that ſo ſimple an operation could have produced ſuch extraordinary appearances? and that thoſe appearances, ſimple as the operation is, ſhould have eſcaped the obſervations of philoſophers, and thoſe particularly who have intereſted

themſelves in theſe matters for ſo many years paſt? But experience teaches us daily, that, during an over eagerneſs in our purſuits to attain ſome favourite point, we generally overlook others of a more extraordinary nature. Perſeverance, in the end, however, ſometimes makes great amends for thoſe overſights, by diſcloſing gradually the the object we are in ſearch after; but it muſt be acknowledged, that chance is frequently more generous, by removing the veil at once, when we the leaſt think of it. For my part, I have been greatly obliged to chance upon this and many other occaſions; and, had it not been frequently taken notice of before, I ſhould have enumerated ſome of the greateſt diſcoveries that have been made by others entirely through accident.

In this work we have ſhewn, by a great variety of experiments, how the priſmatic colours may be exhibited in the dark as phoſphori. Why may not chance hereafter, by ſome lucky combination of circumſtances, again produce ſome diſcovery, in conſequence of thoſe extraordinary appearances, that may turn out to be of far greater importance? Attention and induſtry ſeldom fail of producing ſomething worthy the notice of philoſophers.

IT now remains to ſay ſomething concerning the *nature* of the *light* by which phoſphoric bodies are ſeen to ſhine in the dark.

The few experiments and obſervations I have to offer upon this ſubject, are thrown out merely for the conſideration of more able obſervers, who may hereafter be diſpoſed to inveſtigate the cauſe of this luminous property, as I know very well, from experience, that even diſtant hints are ſometimes attended with very good conſequences.

In the firſt memoir by Beccari, there are other ideas reſpecting this matter, which to me appear highly deſerving of attention. But to enter upon thoſe curious ſpeculations in a manner they deſerve, requires greater abilities than I am maſter of. The little knowledge I have in theſe matters is founded upon experiments of my own, and Sir Iſaac Newton's Obſervations upon the nature and action of light and fire, and the ſulphureous or inflammable principle deſcribed in his optics.

All ſubſtances appear to contain the inflammable principle more or leſs; and thoſe ſubſtances which abound with it moſt grow hot, and kindle into fire and flame more readily by the action of light, or other violent motions, than other bodies do.

This principle however does not, in all caſes, appear to be equally diſtributed through, or equally united to, the ſeveral parts, in all bodies.

In phoſphoric bodies, for example, the inflammable principle appears to be ſo weakly united, and even in thoſe inſtances where ſome parts contain more of it than others, as in the ſhells we have experimented upon, that it is more liable to be diſengaged, and emitted from them, in conſequence of the action of light or other violent motions, than in other inſtances where it is more ſtrongly united.

And that the phoſphoric light depends in a great meaſure upon this principle, and not entirely upon the actual ſolar rays which the ſhells had previouſly imbibed, appears from ſome experiments I made in conſequence of reading a curious one related by the celebrated Father Beccari, of Turin, in the Tranſactions of the Royal Society for the year 1771 *; and which I have repeated many times in the manner in which it is deſcribed; and afterwards with ſome conſiderable variations. But, whether from not knowing ſome material circumſtance which he poſſibly may have omitted, or I have overlooked, the experiment, in my trials, did not

* Thecas plures confici curavi ex lamina ferrea cylindraceas intus nigerrimas. Operculum late pertuſum cryſtallo occluditur, colore in theca quaque diverſo. Singulis thecis offas immiſi ex phoſphoro calcareo-ſulphureo ſingulas omnia pares. Hæ clauſæ ſoli objiciuntur ſimul omnes; aſportatas in tenebras aperio, atque offam, quæ per cryſtallum viridem, video vireſcere; rubeſcere, quæ per rubram; flaveſcere, quæ per flavam cryſtallum lucem imbuit: videlicet conſit hoc experimento jam non quantum ſolum lucem ebiberit phoſphorus, ſed et qualem, eam ipſum unice emittere.

not anſwer in the manner in which he has deſcribed it.

This learned father tells us, that he procured ſeveral cylindrical boxes of tin, which were black on the inſide. The top of each box was covered with glaſs of a different colour. In each of theſe boxes he put cakes of ſulphureous calcareous phoſphori which were like each other in all reſpects; and after they were incloſed, he expoſed them all to the ſun at the ſame time: and then opened the boxes, after having brought them into the dark. When he perceived that the cake which had imbibed the light through the green glaſs, was green; that that through the red, was red, and that through the yellow was yellow. From this experiment he not only obſerved how much light the phoſphorus imbibed, but that it emitted only ſuch as it imbibed.

Dr. Prieſtley, after giving an account of F. Beccari's experiment, proceeds in theſe words: "This therefore ſeems to be deciſive in favour of "the opinion, that the phoſphorus emits the very "ſame light that it receives, and no other, and "conſequently that light conſiſts of real particles "of matter, capable of being thus imbibed, re-"tained, and emitted; and I doubt not but that "Zanottus, (if he be now living) will *retract* "his concluſion."

Now Zanotti's concluſion, by the ſame author, (for I have not ſeen the original) is, "that phoſ-"phori ſhine by their own *native light*, after they "have been *kindled* by foreign light."

I am

I am very happy in having this opportunity of mentioning that the learned Zanotti ſeems to be much nearer the truth than Dr. Prieſtley apprehends: and perhaps he will have no reaſon to retract his concluſion, after reading the following experiments and obſervations; or at leaſt till they are contradicted by other experiments *.

1. My firſt attempt, in repeating Father Beccari's experiment, was with cylindrical glaſſes of different colours, that were open at one end, and which I covered occaſionally with three pieces of black leather. The depth of each glaſs was about ſix inches, and the width about two inches. Into theſe glaſſes I put, at different times, the ſame piece of oyſter-ſhell which I had previouſly prepared, and which in the dark gave a very fine white light, that continued above fifteen minutes. I then tied the leathers over the aperture of the glaſs I made the experiment with; and, as each glaſs had a little bending border round the top, which projected outwards, it was the more convenient to exclude the light by tying the leathers cloſe below this border. The advantage I gained by having glaſſes inſtead of metal boxes was, that the ſhell within was more expoſed to the ſun than in Beccari's method. And that little time might be loſt by untying the leathers, I tied them with a broad piece of tape, and in ſuch a manner, that one motion was ſufficient to leave the leathers at liberty. In all the trials I made with theſe different coloured glaſſes, viz. red, yellow, green, and blue, and with the ſame

* Prieſtley on Viſion, &c. p. 365.

ſame ſhell, there was no other appearance than a white light: but that light was a little fainter than the phoſphoric light produced by expoſing the ſhell to the open day.

2. I now had recourſe to cylindrical boxes that were black on the inſide, into which I put the ſame ſhell in each experiment, and then covered the boxes with different coloured glaſs, in the manner Beccari has deſcribed. After this they were expoſed to the ſun, but at different times. When I uncovered the box, by removing the red glaſs, (which was the firſt experiment) the ſhell exhibited the ſame kind of white light I had obſerved with the cylindrical coloured glaſſes. Neither was there any difference in the appearance of this light, when the green, yellow, or blue making rays were tranſmitted ſeparately through the reſpective coloured glaſſes in the other boxes upon the ſame ſhell.

3. But leſt any objection might be made to the nature of the phoſphoric ſhell I employed in the laſt experiment, I repeated them with the ſulphureous calcareous phoſphorus which Mr. Canton gave an account of to the Royal Society, and which ſeems to have been the ſame that Father Beccari made uſe of, without being able to obſerve any alteration in the appearances from thoſe I have deſcribed above.

Seeing ſo great a difference in Beccari's obſervations, compared with theſe; and leſt any doubt ſhould remain between our experiments, I had a mind to try whether it might not be removed

moved by varying the experiment in ſuch a manner as to become abſolutely deciſive.

4. For this purpoſe, inſtead of employing the ſhell which gave the white light, I put in it's place another ſhell, which would exhibit the priſmatic colours intenſely bright after it had been expoſed to the ſun. By this alteration I imagined, that if the coloured glaſs made any material difference, it would be perceptible upon ſome one or other of the colours. The firſt experiment was made with the cylindrical box, and a fine piece of red glaſs, ground and poliſhed on both ſides, and tinged equally with red throughout. On removing this glaſs in the dark, after the ſhell in the box had been expoſed to the ſun, the priſmatic colours appeared; and I could not perceive any alteration in them, except that they were fainter; but I think the red was the leaſt ſo.

The ſame effect of colours, with the laſt exception, was produced with all the other coloured glaſſes: that is, when the yellow glaſs was made uſe of, the yellow light ſeemed to appear a little brighter; when the green, the green; and when the blue, the blue: the other colours reſpectively continuing much the ſame as with the red glaſs in the laſt experiment.

Now, as all the colours were exhibited on the ſame ſhell in theſe experiments, by expoſing the ſeveral boxes to the light with their reſpective glaſſes, thoſe appearances muſt have depended upon ſome other principle as well as light: becauſe the red

red glaſs tranſmitted only the red making rays. And that principle muſt have been the phlogiſton which was previouſly lodged within the ſhell.

If therefore we conſider the nature and diſpoſition of thoſe parts which conſtitute the ſhells, after they have been phlogiſticated, they can emit no other coloured light than what correſponds with the thickneſſes and denſities of their parts: for example, the denſeſt parts (provided they are ſufficiently ſo) cannot emit any other colour than red, no more than thoſe which are next in degree of denſity can emit any other colour than yellow; and ſo on.

When therefore one of theſe ſhells happens to be of an uniform denſity, the whole ſhell ought to emit but one colour; anſwering, as I ſuppoſe, to the thickneſs and denſity of the parts. And the experiment with the leaves of gold, where the whole ſhell nearly appeared of a red colour, ſeems to confirm the truth of the obſervation. For the gold in that experiment which imparted the phlogiſton to the ſhells, may be ſuppoſed to have been of an equal thickneſs, and the contact of it alſo pretty nearly equal.

And to ſhew that light is a neceſſary inſtrument in producing the priſmatic colours, as well as many other phoſphoric appearances, we will now relate ſome experiments that were made by the application of heat.

Having procured ſeveral fine ſpecimens of ſhells that gave the priſmatic colours intenſely

bright, I took three of thoſe ſhells, and laid them flat, and at a little diſtance from one another, upon a tin-plate, which covered a red hot maſs of iron about five inches ſquare, and two inches thick. This iron was ſhut up in a box of tin, to prevent the light from the heated iron diſturbing my eyes. But this precaution proved of little uſe, becauſe the tin-plate itſelf, before I begun the experiment, acquired a red heat in two or three different places which were neareſt to the iron. In a very ſhort time, after the ſhells were laid upon the tin, a feeble whitiſh light appeared at the edges of the ſhells; this afterwards ſpread upon their upper ſurfaces; and it increaſed in brightneſs for a little time in almoſt every part, but particularly at the edges. The whole time taken up by this increaſe, was not leſs than half a minute. Soon after the brightneſs began to decreaſe, and continued to do ſo till it entirely vaniſhed. But this decreaſing of the light was three or four times longer than the increaſe; and the ſplendor at beſt did not exceed that produced by the external light upon the ſame ſhells in a cloudy day. In this experiment I perceived ſome of the priſmatic colours, but then they appeared more obſcure, and conſiderably fainter, than what the open day produced upon the ſame ſhells, particularly the red.

6. When theſe ſhells were become quite cold, and without expoſing them again to the ſun, I laid them, as before, upon the ſame heated tin: and

and though I waited ſeveral minutes, there was only ſo ſmall an appearance of light from the ſhells, that it was with ſome difficulty I could diſtinguiſh it.

This laſt experiment ſeems to prove what we obſerved before, that light is a neceſſary inſtrument to produce thoſe kind of phoſphori. But this, perhaps, will appear more evident from the following experiments.

7. Whilſt thoſe ſhells continued hot, they were expoſed to the open day. Upon bringing them into the dark, I obſerved, that the priſmatic colours appeared ſomething brighter than when they had been expoſed to the ſun in a cold ſtate. I therefore waited a little till they recovered the ſame ſtate as to cold, and then expoſed them again to the light: the colours, after this, were revived, and appeared very lively, but not quite ſo bright as when the ſhells were hot. I repeated the 6th and 7th experiments many times, with the ſame ſhells, and always obſerved the ſame appearances reſpectively, as are deſcribed in theſe experiments.

By theſe laſt experiments it appears, that Dr. Prieſtley was not right in his conjecture, where he ſays, " I would beg leave to obſerve, that the *ſame* " *ſtate of heat*, which diſpoſes the phoſphorus to " throw off the light after it has been imbibed, " *muſt likewiſe render it indiſpoſed to receive it*; ſo " that, in thoſe circumſtances, *it will in reality* " *imbibe leſs light, as I doubt not would be found to*

" *be the caſe*, if the head of the obſerver was " cloaked up, and the phoſphorus preſented to " him immediately after being expoſed to the " light." *

8. The ſame ſhells were now expoſed again to the open day in a cold ſtate; and whilſt they continued ſhining in all their ſplendor of colours, I placed them upon the hot tin as before, and with that ſide upwards on which the colours appeared moſt vivid. This took up ſo much time, that they became ſomewhat fainter. However, the heat they were now expoſed to, revived the colours conſiderably, eſpecially the red, yellow, and blue. And when the colours diſappeared, a whitiſh light occupied their places, which laſted about one minute before it entirely vaniſhed.

9. I now applied the edge of one of thoſe ſhells (which was rather ſmooth than otherwiſe) to the heated tin, and at the ſame time held the ſhell in a pair of pincers. In a few ſeconds, a pale whitiſh light appeared at the edge next to the tin, which ſpread by degrees to the top of the ſhell. This light increaſed in brightneſs at the bottom, and ſpread itſelf upwards a full inch: beyond that, the light decreaſed gradually, and in a little time totally diſappeared. During the decreaſe of this light, and ſome time before it became very weak, a darkneſs took place next the edge in contact with the tin; which ſpread upwards upon the light, more than half an inch. In

* Prieſtley on Viſion, &c. 377.

In a little time after this, the whole phoſphoric light diſappeared by degrees. I continued this ſhell in the ſame poſition upon a heated iron for ſome minutes longer; when I obſerved another light cloſe to the ſame edge that was next the tin, and now next the iron; but this ſecond light was very weak, and did not ſpread farther than the twentieth of an inch.

10. I repeated theſe experiments with other ſhells of different thickneſſes, to obſerve the appearances on both ſides of each ſhell. With theſe I found, that one ſide ſeemed generally brighter than the other: but the whitiſh light, and the feeble colours that appeared, (for they were barely diſtinguiſhable) did not ſpread over both ſurfaces entirely, at leaſt with ſome of them; there being many places on one ſide that did not give any light, and which I ſuppoſe aroſe from a different texture of the parts.

During the making of theſe experiments, I frequently examined the edge itſelf, which was in contact with the tin. This appeared luminous before any other part of the ſhell; and afterwards dark before the half inch of the ſhell became dark, deſcribed above.

From theſe experiments it appears, that the phoſphoric light and colours (ſuch as we found them) are exhibited, and not generated by heat.

It alſo appears, by the preceeding experiments upon light, that the red tranſmitted rays from their nature, were incapable of being ſeparated,

and

and emitted from the ſhell, ſo as to exhibit all the other colours.

Thoſe colours therefore muſt have depended upon ſome other principle, which was capable of making ſuch a kind of partial change among the parts, as was not of a laſting nature: becauſe, in ſome circumſtances, the phoſphoric appearances are not renewable by the ſame cauſe, after they have been once expelled by it. On which account this change, I ſuppoſe, cannot ariſe from any other cauſe, than the mutual action between light and the inflammable principle; for, according to Sir Iſaac Newton's obſervations, "the "changing of bodies into light, and light into "bodies, is very conformable to the courſe of "nature, which ſeems delighted with tranſmu-"tations."

Now by comparing the different experiments together, where light and heat were employed, the phœnomena produced by each being ſo very different, are ſcarcely reconcileable with one another, without ſuppoſing ſuch a *partial* and *temporary* tranſmutation between the inflammable parts and the *matter* of light. For if this ſuppoſition has no foundation, I would aſk, how it happens, that the red making rays exhibit all the other colours in the ſhells? and why heat exhibits a phoſphoric light, and a weak appearance of colours upon the ſhells in one caſe *, and little, or none at all, in another? † And yet the ſame ſhells

* 5th Experiment.
† 6th Experiment.

ſhells in either of theſe caſes, at the inſtant after expoſing them to the light, only for half a ſecond or leſs, give the priſmatic colours afreſh, as if no alteration had happened. And theſe colours, after they have ceaſed to ſhine, are renewable again and again: for the experiment may be repeated a whole day together, ſo far as I have obſerved, whether the light they are expoſed to conſiſts only of the red, or yellow, or green, or blue making rays. This being the fact, the difficulty of reconciling the appearances ſeems inexplicable, except upon the principle which I have aſſigned.

It appears no leſs difficult to account for the emiſſion of phoſphoric light, without having recourſe to a ſubtile and elaſtic fluid, in which vibrations are excited by the action of light, or heat, or other violent motions; for the conſequence of ſuch vibrations muſt be an agitation of the phoſphoric parts; and, when they are ſufficiently agitated, I ſee no reaſon why they may not emit light, and ſhine, according to the circumſtances which accompany them.

Whether the air is at all concerned in theſe luminous appearances I have had no opportunity of examining. The difficulty of procuring what is generally underſtood to be a good vacuum, was one diſcouragement to my attempting any thing of the kind: but there was another which I had experienced, reſpecting the different appearances by different approximations to a vacuum. For,

from

from ſome experiments I made many years ago, in company with Mr. Smeaton, and with his excellent air pump which rarefied the air above two thouſand times, we obſerved in general, that very ſmall differences of air occaſioned very material differences in the luminous effects produced by the electric fluid; inſomuch, that when all the air was taken out of the receiver which this pump at that time was capable of extracting, no electric light was perceptible in the dark; but, upon letting in a little air by a ſtop-cock, a faint electric light was viſible, and the letting in of more air increaſed the light conſiderably. But this light began to decreaſe on the letting in of more air, till, at laſt, on letting in greater quantities, it intirely vaniſhed. By this experiment it appeared, that a certain denſity of air was neceſſary to occaſion the greateſt luminous effects.

This diſcovery induced me to ſuſpect, that the vacuum in the Torricellian tube is not a perfect one; becauſe, when the tube is properly electrified, a ſimilar light to the brighteſt deſcribed above is viſible in the dark through the whole length of the vacuum.

I have introduced theſe obſervations only to ſhew how careful we ought to be in drawing general concluſions from experiments which are liable to many exceptions.

The mentioning of electric effects gives me an opportunity of introducing an obſervation I made

many

many years ago concerning that fluid. And it is the more proper to take notice of it in this place, as it relates to the inflammable principle. The obſervation is this: upon making a variety of experiments relative to the electric fluid, I obſerved, that the luminous appearances were occaſioned by a ſulphureous principle that was driven off from bodies in conſequence of the violent motion proceeding from that ſubtile and elaſtic fluid; and which ſulphureous principle, by mixing with the acid particles in the air, kindles into flame.

From obſerving how eſſential the inflammable principle is, in occaſioning phoſphoric appearances, I was naturally induced to make ſome experiments upon the electric fluid with the ſhells; and, though I found ſome encouragement to apprehend that the matter which occaſions thoſe luminous effects in electricity depends upon this inflammable principle, yet I forbear giving any account of the experiments till ſome others have been tried, which, from their nature, may prove more concluſive than thoſe I have already made.

POSTSCRIPT.

IN page 61. it is obſerved, that had a proper opportunity offered, I ſhould have calcined ſome ſhells with light, congregated by a burning glaſs, with a view to have tried whether they would exhibit phoſphoric appearances, and particularly the priſmatic colours.

Since that time I have met with ſeveral favourable opportunities of calcining oyſter-ſhells by the rays of the ſun, collected with concave mirrors of different dimenſions, the largeſt being about two feet diameter.

On expoſing one of the oyſter-ſhells, which I held with a pair of tongs, in the focus of the laſt mirror, a vapour from the ſhell, in a very ſhort time, began to ſhew itſelf in conſequence thereof, which continued for ſome time, and frequently burſt forth in great quantities, throwing ſeveral ſmall parts of the ſhell along with it. Some of thoſe parts appeared of different colours as they happened to paſs the ſolar rays, (collected by the mirror) and were differently heated. At the ſame time there iſſued from this ſhell (as well as the other ſhells that I afterwards tried) a watery kind of matter, which fell upon the floor in drops. When I thought that part of the ſhell, to which the

the rays were applied, was ſufficiently calcined, I ſhifted the ſituation of the ſhell to calcine a different part. By repeating this operation, I at laſt accompliſhed the calcination of four fifths of the ſhell at leaſt; I mean on the inſide thereof. After this, I expoſed the back part of the ſhell to the rays in the ſame manner, but not for ſo long a time in any part of it. The longeſt time I expoſed any part of the ſhell to the rays, in all theſe trials, did not exceed three minutes; and the ſhorteſt about twenty ſeconds. I obſerved the ſame rule nearly with all the other ſhells, which I calcined afterwards. Being thus prepared, I took the ſhells into the dark cloſet, and after waiting a proper time, they were expoſed to the light of the ſun. On examining them in the dark, I obſerved on the inſide of one ſhell, a very good red, about half an inch in diameter; a little yellow, and a great deal of tender blue. In two or three parts there was a faint appearance of purple. And at the back of the ſame ſhell there appeared two broad tender blues, and a little purple between them.

On the inſide of the ſecond ſhell, the red and yellow were very vivid; there was alſo a good green; but the blue was very faint; and if there was a purple, I did not obſerve it: this, however, was not the caſe on the oppoſite ſide or back of the ſhell, for the purple predominated over a moderate red and blue light.

By

By the ſame method I had calcined three other ſhells, the colours they exhibited were pretty nearly the ſame with thoſe I have now been deſcribing, except that in two of them there was no appearance of purple; and in one, no green; but then there was ſome red and blue, with a great deal of white light in various parts.

FINIS.

Keith. prey

AN

ACCOUNT

OF A GREAT NUMBER OF

PHOSPHORI,

DISCOVERED BY

J. B. BECCARII,

Professor of PHILOSOPHY in *Bolonia.*

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1. OF bodies that give light in the dark there are several kinds. For some bodies throw out light *spontaneously*, and others upon being *excited*. And of the former kind some shine with a *natural light* as glow-worms, dates, and a good many aquaticks: others enjoy an *adventitious light*, as rotten woods and the flesh of some quadrupeds and birds. These last are not naturally phosphori, but owe that property to some particular cause, which

generally is *putrefaction*, and ſometimes an inſenſible change introduced into the natural conſtitution of the parts.

2. The other kind of bodies which become phoſphori upon being excited, or that are at leaſt aſſiſted by that means, may be diſtributed into different ſpecies, according to the different modes of *excitation*. Theſe modes are *attrition*, *heat*, the *free admiſſion of the air*, and laſtly, *being expoſed to the external light*.

3. Bodies of every kind become phoſphori by *attrition*, provided they can bear that force of friction which is ſufficient to produce the reluctant light that is hid in their ſubſtance. For the ſame reaſon almoſt all bodies grow hot, and often ſo violently as to take fire. There are ſeveral modes of *attrition*: in the firſt place *preſſure*, whereby, as Du Fay obſerved, ſome diamonds became phoſphori: in the ſecond place, *colliſion*, which is the vulgar method of ſtriking fire out of ſtones: *friction*, and *contrition*; which laſt is the well known method whereby the coats of many animals are made to emit ſparks. In the laſt place, and not to be too minute, we muſt reckon *agitation*, which agrees moſt particularly with *liquid* ſubſtances: as the ſea water when ſtruck with ores, and agitated in a dark night, is always obſerved to ſhine.

4. The emerald phoſphorus and many gems, and amongſt theſe not a few diamonds, together with many other ſtones of ſmall value, as the lapis lazuli, the falſe emerald, and a great part of the

the mountain cryſtals, become phoſphori by the application of *heat*.

5. But the *free admiſſion of air*, in uſing the phoſphorus of Konkel or rather of Kraft, not only produces light, but even a blaze of fire, where friction is *uſed*. The phoſphorus of *Homberg* and a great many others formed upon the ſame principles from *allum*, or any other animal or vegetable ſubſtance, according to Lemmery's method, as well as Le Fevre's preparation from iron and ſulphur, burn very furiouſly upon the approach of the air. In the laſt place we muſt mention the *pulvis fulminans* of Godofred, which exceeds every thing of this kind, and has obtained that name from the thundering report it makes, and the quantity of ſparks it emits upon the admiſſion of air.

6. In this ſecond kind of bodies the *Bolonia ſtone* deſerves the principal place. From this the philoſophers were firſt taught this wonderful faculty of *attracting the external light*, which was before entirely unknown; by which means they learned to inveſtigate a ſimilar power in other bodies. But their attempts were long without ſucceſs: and if accident had not aſſiſted them, perhaps our Bolonia ſtone had been the only phoſphorus known at this day.

7. For though *Helmont* has told us, in his writings, that he had in his poſſeſſion a *flint* ſo prepared as to receive the light, and to preſerve it viſible for ſome time in a dark place; yet nobody ſince Helmont has been curious enough to find out

his preparation. Whence it has happened that the ſecret of making this phoſphorus, and almoſt the memory of it, have died with the inventor.

8. In conſequence of this, the Bolonia ſtone maintained for ſome time a conſiderable reputation, until *Chriſtian Adolphus Baldwyn*, in ſearching after ſome ſtrange chymical menſtruum, diſcovered a phoſphorus which perhaps he was not looking for, and certainly had no hopes of finding. This phoſphorus was like *our* ſtone in it's faculty of attracting the external light, and not inferior to it in the brilliancy of it's ſplendor. It was therefore received by the philoſophers with ſingular admiration and pleaſure. For as there was not enough of the Bolonia phoſphorus to furniſh every body with ſpecimens who wanted them, (which was not owing to the loſs of the art of making them, as is generally believed, but becauſe this ſecret was confined to a few families at Bolonia, and principally to the family of Zagony) this new phoſphorus came very opportunely to ſupply it's place, and was eaſily procured by all that wanted it, and was as fit for making the experiments as the Bolonia one. Though after all, it is uncertain, whether the phoſphorus which is now generally aſcribed to Baldwyn, and compoſed of chalk, diſſolved in ſpirit of nitre, be the ſame phoſphorus which that author ſeems to have hid by a number of dark paſſages, rather than diſcloſed.

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9. After the diſcovery of *Baldwyn*'s phoſphorus, no addition was made to this claſs for near 60 years. But in the year 1730 Du Fay introduced an incredible number. For this ingenious perſon found that whatever ſubſtances might be turned to a calx by burning only, or being diſſolved in a nitrous acid, and then brought into a ſolid form again, that was hard enough to be burnt to a white heat, were proper to imbibe the external light. By this means the phoſphorus, which was before a ſingle genus, and confined to the *Bolonia* ſtone, was much increaſed, and divided into two claſſes; in one of which our phoſphorus obtains the firſt place, and in the other the phoſphorus of *Baldwyn*.

10. From theſe fortunate beginings he imagined and expected, that all bodies might be made capable of this diſtinction, either by the proceſſes before mentioned, or perhaps by ſome one more obvious. True it is that Du Fay uſed his utmoſt endeavours to bring into this claſs of phoſphori, metals, gems, chryſtals, flints, and other bodies of that ſort which could reſiſt the force of fire, or of corroding liquors.

11. But what he endeavoured to reach with ſuch great labour, he found four years after effected by nature herſelf in the moſt noble of all bodies. Du Fay was at that time examining electric ſubſtances; and obſerving in moſt of them, that not only an attractive power, but even light was excited by friction, he made a more diligent enquiry into the wonderful differences of that light.

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Upon this occaſion he made many experiments of his own invention, and at the ſame time repeated thoſe which *Boyle* had formerly made with *Clayton*'s diamond. Amongſt theſe he diſtinguiſhed that experiment in which the gem when moved towards a *lighted candle* continued to ſhine for ſome time with the ſplendor that remained. *Boyle* had been of opinion that this light was owing to the heat occaſioned by the vicinity of the flame: and Du Fay upon firſt making theſe experiments thought the ſame. But he afterwards expoſed ſome diamonds to the rays of the ſun, and though they acquired very little heat, yet they had juſt the ſame ſplendor: upon which he changed his opinion, and determined that this light did not proceed from the heat, but from the light of the ſun. And here he found himſelf engaged in a very large field of enquiry. Therefore he began in the ſame manner to make experiments firſt upon the other kinds of gems, and afterwards upon other ſtones, as well precious, as otherwiſe. But theſe purſuits did not repay his labour, or anſwer his expectation. He found indeed that ſome diamonds, and a certain rough emerald, together with certain cryſtalline concretions reſembling emeralds, amethyſts, and topazes in their colour, but much inferior to them in hardneſs, would ſhine in the dark after being expoſed to the light. To theſe he added the lapis lazuli, the Bern ſtone, and a few leaden ores. Except theſe bodies he could diſcover none that might be reckoned amongſt gems for their hardneſs and brilliancy, or

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that might be deemed related to cryſtals, or talks, or ſelenites, or chalks, or even to the numerous claſſes of marbles, that had this property.

12. Whilſt this was doing at Paris, I accidentally diſcovered the ſame wonderful power in a diamond which I happened to wear in my ring. It appeared to me a ſubject well worthy the attention of a philoſopher. I immediately began to try all ſorts of experiments, in different kinds of lights, upon my own diamond, and upon as many others, of different ſizes, waters, tranſparencies, and colours, as I could procure, in order to diſcover what difference might ariſe in this variety of ſtones, either in the mode, force, or continuance of the ſplendor. I communicated theſe enquiries, and the whole fruit of them, ſuch as it was, to the academy. And I received from that ſociety the moſt favourable marks of their approbation. However many things were left for further enquiry, and it was indeed impoſſible even to touch upon ſome parts of the ſubject. In particular, no enquiry was made into the cauſe why ſome diamonds become phoſphori, whilſt others in all reſpects ſimilar to them, and expoſed to the ſame light, do ſtill remain dark.

13. This new inveſtigation brought with it more labour than profit, with reſpect to the principal point; but afforded a great deal of matter which was little expected from it. For being obliged in the courſe of theſe experiments to adopt a very nice method of obſerving, inaſmuch as the ſmalleſt difference of light would have

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very material conſequences: it followed from this accuracy that ſome things were obſerved, which had before eſcaped not only me, who am a pretty diligent obſerver, but likewiſe other perſons of great ſagacity and diſcernment. I found for inſtance, with reſpect to diamonds, upon account of which bodies this enquiry was undertaken, that there are amongſt them a much greater number of phoſphori than any perſon of leſs attention would have imagined. It appeared too, that beſides this gem, and the ſmall number of ſtones above-mentioned by Du Fay, that many other bodies are capable of the ſame diſtinction without any artificial means and by the mere liberality of nature; and that not a few other ſubſtances become phoſphori by undergoing a very ſmall alteration. Being therefore entered into this large field of enquiry, I thought it my duty to conduct with the utmoſt diligence and care what I had undertaken by accident. For this reaſon I employed upon theſe experiments all the time that could be ſpared from more ſerious buſineſs. And finding that I had ſpent as much time upon this ſubject, with a view of extending my enquiries, as could be expected or required from a perſon who had many other engagements; and moreover that it would take a much longer ſpace to bring them to perfection: for theſe reaſons I reſolved not to keep theſe obſervations private, but to communicate them, how trifling ſoever, to the learned. And I hope that ſuch as are judges of the ſubject, if they find my obſerva-

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tions to be founded upon truth, will not altogether deſpiſe the preſent undertaking; eſpecially as they muſt know, that in phyſical learning many great and uſeful diſcoveries have ariſen, or at leaſt have been advanced, and perfected, from circumſtances apparently of ſmall moment.

14. But before we ſpeak of thoſe bodies that are obſerved to ſhine in the dark by means of the light they have before imbibed, it will be proper to explain in what manner the obſerver ſhould prepare himſelf for this purpoſe; ſince the whole affair depends almoſt entirely upon ſuch preparation. It muſt be underſtood that the light imbibed by moſt of theſe bodies is neither very bright, nor very laſting. It is very clear that a weak light does not ſufficiently ſtrike the eye that has been before accuſtomed to a ſtronger; and it is as certain, that a tranſient and ſhort-lived light will be extinguiſhed before the commotion of the eye, occaſioned by the precedent ſtrong light, can have time to ſettle. In order to prevent theſe inconveniencies it is proper in the firſt place, before we begin our obſervations, to remain in a very dark place until every veſtige of the light before received into the eye is entirely obliterated: and in the next place that the bodies upon which we obſerve ſhould be conveyed from the full light into the dark place of obſervation with the utmoſt ſwiftneſs; which cannot be performed unleſs the place of obſervation be very cloſe to the full light. But in this caſe how ſhall we avoid admitting ſome

portion of light ſufficient to diſturb the obſerver's eye, together with the body? However, I manage this point in the following manner: others muſt ſuit their own fancy and convenience, but this contrivance anſwered my purpoſe. I had a

ſmall box made, but large enough either to ſit or ſtand in, and at the ſame time of a convenient ſize to be moved about. There was a door on one ſide, which ſhut very cloſe; on the other ſide was a window cut out, to which was fixed a cylinder or *tympanum*, ſo cloſely joined as to leave the leaſt ſpace poſſible between the joints, namely juſt enough to give room for the cylinder to turn round it's axis, which was perpendicular. To the borders of the window were fixed two wings, and to the upper and lower borders were fixed two liſts: all of which were of ſuch breadth, and ſo adapted to the curvature of the drum, as to intercept ſuch portion of the external light, as would otherwiſe have paſſed through the joinings. In the laſt place there was an aperture from the top to the bottom of the cylinder, being in breadth about the ſixth part of it's circumference. Through this opening the ſeveral bodies were placed upon the bottom of the cylinder, and by that means they were eaſily expoſed to the light or to the obſerver, by only turning the cylinder. And thus were thoſe bodies conveyed in the ſwifteſt manner from the light into the dark; and the obſcurity was at the ſame time defended entirely

entirely from the light. Nothing therefore remained but that the ſpectator ſhould have his eye as free as poſſible from every preceding impreſſion. And this precaution requires a conſiderable time to be paſſed in the dark: which circumſtance would be very diſagreeable even to a man at perfect leiſure, and to a perſon anxious to try new experiments, muſt be exceedingly diſagreeable. Therefore Du Fay is very right in recommending the firſt part of the morning for theſe obſervations, ſince the reſt and darkneſs of the night have then refreſhed the eye, and given it it's full ſtrength. As ſoon therefore as a perſon wakes, he ſhould immediately apply himſelf to theſe obſervations without any previous preparation. But if it ſhould happen that the day-light has firſt fallen upon the eyes, and the obſerver ſhould be unwilling to go through the previous preparation of darkneſs and ſolitude; in that caſe, Du Fay recommends the keeping of one eye open and the other ſhut. By this means the eye that is ſhut will, by it's reſt and the excluſion of light, be fit for obſerving; and the perſon may uſe his other eye to prepare what is neceſſary for the experiment. Theſe preparations will take up half an hour, or ſomething longer, for in theſe matters delay cannot hurt, but diſpatch may. When this time is over, let him get into his box and prepare for obſervation; for which he may know himſelf to be ready, when he is able in the dark to perceive the form and whitiſh hue of a ſheet of white paper.

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15. Thus much for the manner in which theſe obſervations ſhould be conducted. In which, if I have been too long I hope to be excuſed, eſpecially by ſuch as have upon other occaſions entred upon a work of this ſort, without any body to direct them. And now we ſhall proceed to the obſervations themſelves.

In order to treat of theſe in proper method, we ſhall divide the whole family of phoſphori into *two grand diviſions*; and ſhall treat in the firſt place of ſuch as are naturally phoſphori, and in the ſecond place of ſuch bodies as becomes phoſphori by means of ſome proceſs, which however is ſlight and does not alter their nature. For the phoſphori that become ſuch by elaborate operations, namely, by the intire diſſolution of their natural conſtitution, have been fully treated of by Mr. Du Fay. With reſpect therefore to our firſt diviſion, namely, the natural phoſphori, we muſt conſider the following ſubdiviſions. In theſe we ſhall follow that order which nature herſelf, the only producer of theſe ſubſtances, hath followed in the arrangement of terreſtrial bodies; that is, we ſhall refer all the phoſphori to the claſſes of *foſſils*, *vegetables*, or *animals*.

Amongſt the foſſils we muſt give the firſt place to thoſe bodies which ſeem to conſtitute the ſtructure of the earth, being it's principal parts, or at leaſt that have a ſtrong relation to it's general diſpoſition; and which bodies we find either thrown together in vaſt maſſes, or ſpread through great

great tracts; or laſtly diſpoſed in ſtrata. Some of theſe bodies conſiſt of parts that are ſcarce, if at all, connected together; which is the caſe of collections of earths or ſands: the parts of other bodies are connected and joined together, as marbles, and other ſtones of great bulk.

16. I have already ſeen many earths that are phoſphori, and likewiſe not a few that are dark. The dark ones are for the moſt part of dark colours. Amongſt theſe I reckon in the firſt place black, and dark; and then the following deep colours, firſt red, then purple, then green, and laſtly orange; but the white, the aſh colour, the green, and the yellow, being faint colours, are neither favourable to the phoſphoric quality, nor the contrary. For example, *nuceriana*, the white bole of the gilders, and likewiſe the bole that is called the Great Duke of Tuſcany's, are white bodies and phoſphoric. But at the ſame time there are many other bodies, as white, that are not phoſphoric, and amongſt theſe the Vicetina fullonia. There is the ſame ſort of uncertainty with reſpect to the other qualities. There is for inſtance a great variety in the nature of earths, ſome of which are of a looſe texture, and others thick and cloſe; for which reaſon the former are light, and the latter more ponderous; ſome again are hard, and others on the contrary ſoft; ſome are tough, and others brittle; ſome are lean, and others fat; ſome are rough, and others ſmooth; amongſt

amongſt which latter ſome are ſo ſmooth as to appear greaſy to the touch. But in this great variety I have not been able to diſcover any certain criterion for diſtinguiſhing which are phoſphori. But in moſt earths the light is weak, and eaſily eſcapes obſervation, unleſs the eye be very clear and attentive. The earth that is brought from the Argentarian mountains, and what is brought from Sicily under the name of the mineral Bezoar, ſeemed to excel other earths in the force and duration of their light. The earth that is called the golden virgin earth by Bocconi, exceeds the foregoing not in it's ſplendor, but in it's duration.

17. After the earths we muſt conſider the ſands, not becauſe they are of the ſame nature with the earth, according to the vulgar opinion, but becauſe they occupy large ſpaces, and conſequently make a conſiderable part of the earth's ſurface. But in truth ſands are very minute fragments of ſtones. It is therefore probable that they follow the nature of the bodies from whence they are broke in the circumſtance of being phoſphori or not, as well as in others. For which reaſon I gave myſelf but little trouble about ſands that are black, or of other deep colours, having known by other obſervations that ſuch colours did not admit the light. I therefore applied myſelf to white and other pallid colours; and in fact I think they appear well adapted to the admiſſion of light; for though I found one

or two that were otherwiſe, yet I am doubtful whether this was owing to their natural conſtitution, or to the adheſion of ſome foreign ſubſtance. For that ſand, which is dug out of the hills near Bolonia, and is of an orange colour, will not become a phoſphorus ſo long as it continues tinged with the ocre by which it is coloured; but, when that is waſhed off, the ſand becomes white, pellucid, and a very fine phoſphorus.

18. Next to the ſands and earths we muſt place ſuch ſtones as are of a great bulk. Theſe, being in a great meaſure diſtributed in ſtrata, by their quantity and ſolidity ſupport and embrace other bodies that are looſe and unconnected, and by that means give a certain firmneſs and ſolidity to this globe. — Some of theſe ſtones are of a looſe texture, and a rugged ſurface; others are of a firmer compoſition, and not altogether rough; and, in the laſt place, ſome are of ſo firm a texture as to receive a ſhining poliſh. The ſandy ſtones, which are in the firſt claſs, are pale and languid phoſphori; the Coſolearia, which is in the ſecond claſs, is ſomewhat brighter; and marbles, which are in the laſt diviſion, are almoſt all phoſphori. But amongſt theſe ſuch as are the whiteſt and the ſofteſt are the moſt eminent. Theſe two qualities concur in the alabaſter. But the moſt beautiful of this kind is that *alabaſter* which our ſtatuaries call *cotogni*. Next to this are certain marbles which are as tender as the alabaſter, but are of a deeper colour in particular parts,

parts, and have conſequently dark ſpots intermixed with their light; for in theſe bodies, as in others, colours obſtruct the phoſphoric quality, and the more, the darker they are; which was before obſerved with reſpect to earths. But with faint colours it is otherwiſe; and indeed there is a marble of a tender red, which I have obſerved to be a good phoſphorus, and eſpecially ſome pieces of Spaniſh marble of that fine ſort which is called *brocatelli*. Porphory, ophites, granates, and other hard and dark marbles of that ſort, are either not phoſphori, or their light is ſo uncertain as to be hardly diſtinguiſhable.

19. Next to theſe large bodies we muſt reckon chalk (or limeſtone) which in ſome countries, and eſpecially about Bolonia, does not lie upon the ſurface of the earth, or in ſmall quantities, but is found in great maſſes. To the ſouth of our city are very high hills, conſiſting entirely of limeſtone; and from thence very large veins thereof run through the neighbouring country. The limeſtone that is dug from thence for building is naturally a pretty ſtrong phoſphorus, but becomes much ſtronger by burning. But this adventitious quality goes off in a few days; which agrees with Du Fay's obſervation with reſpect to this ſtone, and to marbles, and other ſtones. But when this artificial light is gone away, the natural phoſphoric quality ſtill remains, and is not deſtroyed by age, or by the injuries of the air, or other ſurrounding bodies. I ſhould

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not have ventured to affirm this, if I had not found a piece of lime mor tr which was taken out of a wall above one hundred years old, and that had preſerved this power quite unhurt by the force of time, and that perpetual ſtreſs of weather to which it was always expoſed.

20. Next to the ſtones which occupy large ſpaces of the earth, by their own great bulk, we muſt ſpeak of ſuch as are in themſelves ſmall, and are not diſpoſed in continued maſſes, but are found intermixed with the ſtrata of other bodies. Some of theſe are not harder than marble, and others are harder than any marble. Of the former kind, ſome have no determinate exterior form, and no particular and certain diſpoſition of the internal parts, which is the caſe of moſt lime-ſtones: the ſecond ſort are informal on the outſide, but have a certain particular diſpoſition of parts on the inſide, which in ſome of them exhibits a grain that runs in parallel lines, as in the amiantus, and the alumen plumoſum: in others it produces thin, flexible, and parallel laminæ, as in talc, and lumps of gold or ſilver: in other bodies it exhibits incruſtations one over another, as in the ætites: and laſtly we muſt reckon ſuch ſtones as have a certain regular formation both internal and external. To this claſs are referred by *Woodward*, who is our authority for this diviſion of the ſelenites, the lapis ſpecularis, the belemnites, the ſtalactites, and oſteocolla.

21. Lime-ſtones are all phoſphori more or leſs. The colour makes a great difference. The white ones are the beſt phoſphori, and the dark ones produce a pale weak light. Of this ſort are thoſe that the public ſtreets are paved with at Bolonia. The amiantus, and the papers that are made from it, are not phoſphori, nor is the talc a phoſphorus; neither the common pellucid ſalt, nor that which is green and unctuous; neither is any of this quality in the ſpangles of talc wherewith the ſands in our mountains are mingled. This power is very weak in ſilver ſpangles; it is ſtronger in a certain kind of ætites which exhibits that aſh colour which is obſerved in the incruſtations of certain ſtones that look like a kind of calx; but there is nothing of it in another kind of ætites that is found in the neighbourhood of *Narni*, and which is a concretion of yellow ocre; ſo that it plainly appears, that in the ſame ſpecies of foſſils there is a great difference in the materials of their compoſition, and conſequently in their phoſphoric qualities. — But in that kind of ſtones which is diſtinguiſhed by a regular external and internal form, nature ſeems to have effected a wonderful ſingularity. In the ſtalactites ſhe amuſes herſelf with the moſt elegant variety of forms. She faſhions tubes, laminæ, trunks of trees, bones, cluſters of grapes, and almoſt an infinite variety of figures, and even ſometimes human forms, out of the ſtrong concretions that are found in water. But in the ſe-

lenites,

lenites, and the Iceland cryſtal, ſhe is more laborious, and conſtructs their figures according to the ſevere laws of geometry. Our Bolonia ſtone has been the object of her induſtry; for ſhe has diſpoſed therein certain ſtriæ, pellucid as cryſtal, and which tend to it's axis in the moſt beautiful manner. Moreover, ſhe has adorned all theſe bodies not only with the greateſt beauty, but with the phoſphoric power. Amongſt the reſt we muſt diſtinguiſh a certain *ſtalactites* that is dug in our neighbourhood, which is rather pellucid, of a yellowiſh colour, and of a beautiful form. Of the ſame kind are thoſe ſtones which are perforated like ſponges, and are therefore called in our language water ſponges. Upon being burnt they afford a very white ſubſtance. Our waters too produce a cruſt on the ſides of the pipes, and at the bottom of veſſels; which cruſt is a very ſtrong phoſphorus. The ſame may be obſerved with reſpect to other concretions belonging to this kind. We need ſay nothing of the ſelenites and the lapis ſpecularis, ſince we have ſaid ſo much of our lime-ſtone, to which they have ſo much reſemblance. We muſt not omit the Iceland cryſtal, and our Bolonia ſtone; which laſt mentioned ſubſtances produce a light almoſt equal to thoſe abovementioned. The belemnitæ, the aſtroitæ, with reſpect to their form, may be reckoned of this kind; but, with reſpect to their origin, they ſeem to be of a quite different one. But whatever kind they may belong to, I

have obſerved that moſt of them are phoſphori, ſome brighter than others. And ſince we are talking of figured ſtones, we muſt obſerve that many dentritæ are phoſphori; but that ſort of dentritæ, that is formed like a croſs and ſpeckled, is little or nothing of a phoſphorus.

22. I come now to conſider ſuch ſtones as are harder than marble. Theſe are all endued with a brightneſs that is altogether ſingular; for they are capable of ſo great a poliſh as to ſend forth light from every point. Many of them are likewiſe ſo perfectly tranſparent that there is no part of them that does not ſeem to be intirely penetrated by the light, and to be full of it. And yet in this kind of bodies, ſo noble and ſo numerous, but a few ſtones can be found that retain in the dark any veſtige of that ſplendor which is ſo great an ornament to them in the light. The lapis lazuli is the only ſtone amongſt the opaque ſtones which preſerves it's light in the dark. The malechitæ, and the whole family of the jaſpers, have not this property. But amongſt the ſtones that have a ſort of tranſparency, I have obſerved that flints have a certain light ſpread over them. And although this is but a weak light, yet it ſeems to penetrate the internal parts of the ſtone, and at the ſame time to aſſume it's colour. Many ſpecimens of agate exhibit a like appearance. In theſe was obſerved a very ſmall light diffuſed through the whole body, and of ſuch a horny colour as this ſtone has. I likewiſe obſerved

ſerved a light ſomewhat ſtronger in ſeveral concretions of the agate kind; and in particular I ſaw one that was formed in a plough, and ſome others in the ſhells of cockles. All of which repreſented, in a beautiful manner, the ſame appearance as the true agate does. But there are a great number of agates that do not ſhew the leaſt appearance of this faint light. But the lapides Caledonii that I have examined are all phoſphori; and, amongſt the reſt, one was ſo bright that the interior parts of it ſhone very conſiderably. But I have found that Du Fay's obſervation, which is mentioned above, is perfectly true with reſpect to all tranſparent ſtones, whether coloured or colourleſs, namely, that diamonds alone are the only phoſphori amongſt the gems; nor does this extend even to all diamonds, nor is the phoſphoric quality in any proportion to the other fine qualities of the ſtone. And with reſpect to cryſtals I have obſerved that their ſize, their tranſparency, or their colour, have nothing to do with their phoſphoric properties; nor are theſe properties affected by the oppoſite circumſtances. And theſe conſiderations have made me ſuſpect that certain concretions which are called cryſtalline, and ſo eſteemed, do in truth differ very much from the mountain cryſtal; ſince otherwiſe they would no more be phoſphori than the true cryſtals are. I have ſeen many ſpecimens of theſe concretions that are as hard as the true cryſtals, and are found in thoſe globular and hollow ſtones

which

which are called *cryſtalline mothers*, and there appear in the form of very pretty ſmall gems. I ſaw likewiſe another cryſtalline concretion in a cockle-ſhell that incruſted it's cavity with exceeding ſmall drops that were moſtly ſexangular. Theſe all were very bright phoſphori.

23. As to thoſe metals which are of the next value to gems, they are farther removed from being phoſphori than gems are; for not only metals, but all ſubſtances that have any affinity to them, are excluded from that claſs of phoſphori into which ſome diamonds are admitted. Therefore cinnabar and lapis calaminaris, and biſmuth, and marcazites, and zinks, and other bodies of the ſame kind, are not phoſphori. But foſſil *orpiment* and white *arſenic* are phoſphori; but perhaps theſe bodies may not be metallic, or they may have ſome particles in their compoſition that are phoſphoric.

24. And if this ſhould be reckoned to be ſome terreſtrial juice, yet I cannot think it to be ſulphur; and yet it is a common opinion that the orpiment has a great deal of ſulphur in it. But I have obſerved that ſulphur, and all ſulphurious bodies, and fat juices, not only ſuch as are liquid, as the *petroleum*, but likewiſe ſuch as are concretions, for inſtance, amber, jet, pit-coal, and all the claſſes of the bitumens, though they kindle by the application of fire, yet they are by no means phoſphori.

25. On

25. On the other hand, *ſalts*, which conſtitute the other claſs of the terreſtrial juices, are phoſphori: but then they muſt be perfectly free from all mixture, and eſpecially of metals: otherwiſe they will not be phoſphori, though they be as tranſparent as the cleareſt gem. I am warranted in this opinion by experiments made upon vitriol, both *Cyprean*, *Roman*, and *German*, and which had been made as tranſparent as a ſaphire, or an emerald, in order for the experiment. Other ſalts are phoſphori: ſome better than others. The rock ſalt and the ſparkling ſalt are very feeble: the opaque ſalt is ſomething better: the foſſil red ſalt not at all: the ſea ſalt is a pretty good one, provided it be cryſtalline and dry: the ſal ammoniac is not extraordinary, but not the worſt of all. The ſalt that is called Cathartic, and the modern preparations of nitre, are pretty ſtrong: the Egyptian ſalt ſomewhat weaker, and ſo is allum: but borax is an exceeding fine and bright phoſphorus.

26. Thus far of bodies which are naturally and truly foſſils. We muſt now ſpeak of ſuch as are reckoned foſſils, not upon account of their origin, but becauſe they are found in the earth. A great part of this claſs conſiſts of the ſhells of marine animals, which are found in ſuch plenty and variety all over the earth; and even at great depths and amongſt the moſt ſolid ſtrata. And theſe muſt needs be good phoſphori, ſince I obſerved that all of them had that quality more or leſs, except

except certain tubes that had had worms in them: and a body of that kind which is called *dentalia*, together with a certain nautilus of the foliatious kind. But none of theſe marine bodies had that quality ſo ſtrong, as ſome gloſſopetræ that I happened of. I have ſeen ſome fiſhes teeth that had acquired the hardneſs of ſtones, and were pretty good phoſphori. The remains of ſome land animals are not altogether without this quality: for I obſerved that the foſſil *ivory* was entirely ſuffuſed with this light; and that many *bones* of quadrupeds were phoſphori at different points of them. But I cannot ſay ſo much with reſpect to the parts of vegetables: if indeed thoſe ſtony pieces which were ſhewn to me under the name of foſſil wood, are properly to be called vegetables: for though they had the form of wood, yet their texture, hardneſs, and colour, were very various. For I ſaw nothing in any of theſe ſubſtances that was not quite dark: though I obſerved them with the greateſt attention.

27. And now we have gone through all the foſſils, and are to ſpeak of the vegetables. This claſs of bodies is as numerous and various in it's ſpecies as any that is known: and yet it ſeems to us to have but few phoſphori. But perhaps I am prejudiced by the incredible multitude of ſplendid minerals that may have dazzled my eyes: and which is ſo great, that I have hardly been able to ſet down their names. However, if I may hazard a conjecture in ſuch an abſtruſe queſtion,

tion, I ſhould think that vegetables are not in fact deſtitute of this power, but that they contain ſome mixture of foreign particles that hinder its appearance. I ſhall give the reaſons for this opinion upon ſome other occaſion. At preſent I muſt obſerve that land plants, whilſt they are full of juice and in verdure, have nothing of the phoſphoric quality. But wood that is dry and without ſap affords a weak and tranſient light: and what may ſeem ſtrange, this light *flows* to the edges and angles in ſuch a manner that it ſeems to leave the reſt of the ſurface. When I firſt ſaw this, it brought to my mind the magnetic virtue which is likewiſe obſerved to flow to the edges of magnetic bodies. It is further remarkable that in ſome pieces of wood, and eſpecially in fir, there are ſmall ſpots about the ſize of a tare, which are of a remarkable brightneſs; and make their appearance in ſuch parts of the body as have nothing to diſtinguiſh them from the general darkneſs of the ſurface. After the woods I examined ſome *barks*, and found them to be middling phoſphori: but the fruits and the ſeeds, together with the kernels and the flour contained in them, had nothing of this quality. And in particular I found that wheat meal and the whiteſt flour of wheat was as dark as any body whatſoever. But it is otherwiſe with that ſoft down which is contained in the fruit of the cotton-tree, and which is produced by it when it becomes ripe: this down is very well known,

and is beautifully white. Salts that are the concretions of the juices of plants, are ſtill better phoſphori. Of this ſort are thoſe ſtones that are found growing to the inſide of wine caſks, and which are called tartar. But of all the ſalts in this claſs of vegetables, the brighteſt phoſphorus is *ſugar*. This is not only bright upon its ſurface, but through its whole ſubſtance, as if it imbibed the light. On the contrary, gums and reſins, as maſtic, myrrh, and frankincenſe, are not at all phoſphori: in which they agree with thoſe fat juices, which we before reckoned among the foſſils. But we muſt except wax out of this account of fat vegetables: it is indeed an oleaginous concretion, but of a ſingular nature. White wax is found to be a phoſphorus, but of a very weak and tranſient kind. As to marine plants, I have very little to offer that is worthy of notice: for I have examined but few, and thoſe not juicy ones, but rather ſtony concretions. I found that ſome white corals and a madripora of the ſtarry kind were phoſphori. But I ſaw another madripora of the fungus kind that was dark.

28. We ſhall now, in the laſt place, conſider of animals. As this claſs of beings excels the other two in many particulars, ſo we may obſerve that it is equal to either of them in its phoſphoric appearances. Indeed it produces ſuch a great number of phoſphori, as one can hardly reckon. But at the ſame time there is ſuch a general reſemblance, and ſuch a natural correſpondence

amongſt

amongſt them, that it ſeems not difficult to compriſe them all under ſome general principles, and to treat of them in a narrow compaſs. It may be obſerved, that whatever animal ſubſtances are ſimply earthy concretions, hardened into a ſolid form, are at the ſame time phoſphori. For inſtance, moſt bones are phoſphori, and eſpecially the bones of quadrupeds; the teeth of animals, and particularly the human teeth, eſpecially in ſuch parts of them as have contracted a ſtony hardneſs, are the moſt phoſphoric amongſt the bones. In theſe bodies hardneſs, compactneſs, and whiteneſs, are circumſtances the moſt favourable to their phoſphoric appearance. For this reaſon almoſt all the ſhells of marine animals are phoſphori; and in the turbinated ſhells the light is the ſtrongeſt at the cavity of the orifice, and in the bivalve ſhells at the nodes of the joints; becauſe, in theſe places, they are the moſt compact, and the whiteſt. Muſcles are covered with a very thin ſhell, and ſo are the *pinnæ*: I have ſeen ſome of this latter kind brought from Amboyna; they had a tranſparent coat, but of the brittleſt kind. Theſe had no phoſphoric quality. The ſame may be obſerved of the unguis odoratus. It's coat is formed out of a bony ſubſtance that is ready to take fire, rather than out of any thing earthy or ſtony. And this makes me ſuſpect, that in ſuch bodies as are in a great meaſure earthy concretions, that the phoſphoric quality is certainly greatly leſſened, if not entirely extinguiſhed,

extinguiſhed, by the mixture of any greaſy ſubſtance. And indeed the horns and hoofs of land animals, which have a great ſhare of oleaginous parts, have either no phoſphoric quality, or it is ſo weak as to be imperceptible. On the other hand, ſtones that are bred in the bodies of animals are pretty ſtrong phoſphori. Of this ſort are the ſtones which are found in the heads of ſome fiſhes, but which the *river aſtaci* have in their belly; ſuch likewiſe are what are called Bezoar ſtones; and laſtly thoſe ſtones that are bred in the kidneys and bladder of men. But perhaps theſe become phoſphoric for a contrary reaſon; for they are lean, and contain no more unctuous matter in them than ſuffices to connect the earthy parts whereof they conſiſt. We find likewiſe many examples of this ſort in other kinds of bodies. We ſhall take the birds as an inſtance. Their feathers, though they are ever ſo white, are not phoſphori. On the contrary, their eggs, or rather the ſhells of them, have a great deal of this quality. I obſerved it particularly in the oſtrich's eggs. But theſe indeed are larger, harder, and thicker, than any other eggs, though there are many as white. But it is more ſtrange, that in thoſe eggs of that foreign bird which they call an *emaw*, and which are thick and hard, but which at the ſame time have a number of rough and dark ſpots upon their ſurface; and likewiſe that in the eggs of many other birds, which, though they are white, have yet

a very thin ſhell, there ſhould be the like phoſphoric quality. But the truth is, that however thin they may be, and however ſpotted, yet their conſtitution is more earthy, and leſs oleaginous, than the feathers.

29. We have now finiſhed our account of ſuch bodies as are *naturally phoſphori*. But before we proceed to the ſecond branch of our general diviſion, namely, the *artificial* phoſphori, we ſhall anſwer a few queſtions that may ariſe upon what has been ſaid. And firſt it may be aſked, in what degree of light, and for how long a time, theſe phoſphoric bodies ſhould be expoſed to it? In the next place, what are the bounds beyond which theſe phoſphoric bodies cannot ſhine, and within which their qualities will not be perceptible? We ſhall next ſhew how long the light may be expected to continue in the dark place. And laſtly we ſhall enquire, whether there is any proportion or correſpondence obſerved amongſt theſe ſubſtances.

30. As to the firſt point I have tried experiments upon different phoſphori in different lights. They all of them are brighteſt in the light of the *ſun*; next to this, in the clearneſs of a ſerene air: a cloudy and dark day affords the weakeſt appearances. The air muſt be free and open; for many phoſphori that would afford a middling light in the open air have been quite dark when ſeen in a light that paſſes through a glaſs window. And therefore *Homberg*, in directing the manner of making

making experiments upon the Bologna ſtone, ſays, very rightly, that it ſhould be placed in the open air. For, if the ſtones are of a dull kind, and not favourable to theſe experiments, the light that comes through the window will not excite them at all, or very weakly. Laſtly, it muſt be obſerved, that the moſt brilliant phoſphori hardly give any light at all, though expoſed to the brighteſt flame. For inſtance, alabaſter, which is perhaps one of the moſt ſplendid phoſphori, if it be expoſed to a large fire, will ſcarcely contract ſo much light as may be found in the common flint pavement of the ſtreets upon a cloudy day. By which we may underſtand what a great difference there is in the force of the ſplendor between theſe two kinds of light, which if conſidered ſeparately, and rather curſorily, may be thought to be a good deal alike, at leaſt in their extremities. For who is there that, in a dark night, when his eye is ſtruck with a bright flame, would not prefer that ſplendor to the cloudy light of a gloomy day, or would not at leaſt think it as good? And yet how many degrees muſt there be between thoſe two extremes of the ſolar light, and the light of the fire, ſince we find that the brighteſt light of the latter is ſo much inferior to what is almoſt the leaſt light of the former, that the objects expoſed to each are ſo differently enlightened? But in truth our ſenſes deceive us extremely in theſe matters, eſpecially when we conſider them ſeparately. Perhaps it would be

ſafer to compare theſe gradations of light by the application of phoſphoric bodies. Thus for inſtance we might, without any great danger of error, determine that degree of day-light to be equal to the greateſt brightneſs of a fire, in which day-light, alabaſter, or any other body of the ſame kind, ſhall be found to ſhine as brightly as in the light of a furnace. But this degree of day-light is not to be found except towards the evening.

31. The time during which bodies muſt be expoſed to the light, in order to make them ſhine in the dark, is very ſhort. Four ſeconds, or five at the moſt, are generally ſufficient for the weakeſt phoſpori, and a longer time is uſeleſs. But to give a middling and tranſient light, it is enough in general to expoſe the bodies to the light for half a ſecond, or a third of a ſecond at moſt. And I muſt obſerve that this *greedineſs* in imbibing the light, which we find in phoſphori, that are not in the firſt, or even in the ſecond rank, is, to my apprehenſion, much more ſurprizing than the great phoſphoric vigour of the Bologna ſtone, which has been ſo much admired by many philoſophers.

32. Thus much ſhall ſuffice, as to the light in which we place our phoſphori, and the time during which they are to continue in it. We are now to ſpeak of the light which they bring with them into the place of obſervation. And as to this it may be aſked what is the ſtrength of it, and how long it continues. But as this is a

ſubject

ſubject that depends entirely upon the judgment of our ſenſes, every body muſt ſee how difficult it is to reduce it to any known and certain ſtandard. For which reaſon we ſhall lay down but one general rule with reſpect to this ſubject, and which is the only one that admits of no doubt; namely, that the light of theſe phoſphori is weaker than that of the Bologna ſtone, and even than that of fine diamonds. For it is certain that this ſtone may be ſeen like a *burning coal* even in a moderate ſhade, and without any extraordinary preparation in the obſerver's eye. And it is as certain, according to Du Fay's obſervation, that diamonds, in order to be viſible, require no greater darkneſs, nor any other clearneſs of ſight, than what are ſufficient to perceive the light that iſſues from rubbed amber. But the phoſphori in queſtion, although they be of the moſt ſplendid kind, and at the height of their luſtre, do yet, to be diſcerned, require that the eye ſhould be more exquiſitely prepared.

33. The time of the duration of the phoſphoric light is, as we before obſerved, not the ſame in all bodies. Some loſe their light ſooner than others; but the longeſt duration of it is very ſhort: for I never ſaw any that kept it's light more than ſix or at moſt eight ſeconds. But there are ſome that do not keep it for two ſeconds. And it is eaſy for an obſerver that is not well aware of this, and ready in his obſervation, to miſtake theſe for dark bodies.

34. From

34. From what has been ſaid it may be inferred, that there exiſts ſome ſort of proportion between the ſeveral parts of this ſubject: the greater the light is, the greater and more durable will be the phoſphoric appearance; which is conſequently leſſened by a ſmall light. The ſame obſervation holds with reſpect to the length of time during which they are expoſed to the light. If this be too ſhort, the light will be feint, and ſoon go out; therefore as much time muſt be allowed as is neceſſary to ſaturate each body with it's juſt quantity of light. This, as we have ſaid, need not be long, but it muſt be enough: the duration of the light is proportionable to the force of the light received; conſequently bodies, that have received a brighter light, do not ſo ſoon become dark as thoſe bodies that have received a weaker light.

35. Having now finiſhed what I propoſed t oſay with reſpect to *natural phoſphori*, I ſhall now proceed to conſider ſuch bodies as become phoſphoric by *artificial* means, or ſome other accidental cauſe; and I ſhall begin with ſuch as are made phoſphoric without the application of fire. The firſt of theſe are ſuch ſubſtances as are produced by the maceration of certain *ſtalks*: and here I mean principally ſuch ſtalks as have barks; or ſuch ſtalks as are furniſhed with fibres that are long, ſolid, and flexible: the only art uſed in this caſe is to get rid of whatever is viſcous, tough, or fat. When theſe dirty parts are removed, the

fibres remain ſoft, flexible, and perfectly white: they are then pounded, carded, ſpun, and wove. And thus cloths are made, thoſe beautiful and uſeful inventions, ſo acceptable to all ages and degrees of human life. But to this uſual commendation of their utility we muſt add, their great merit as phoſphori of conſiderable rank. For it muſt be underſtood, that although flax, or hemp, undergoes ſuch a number of changes, by heat in the ſun's light, by being diſſolved in water, by putrefaction, by waſhing, by bleaching, and ſprinkling; I ſay, notwithſtanding all this, the natural conſtitution and mixture of theſe bodies is not at all corrupted or impaired. For every injury ariſing from ſo long and laborious a proceſs affects not the ſtamina of the plants, but their juices only. It is theſe that ferment, that putrify, and that are cleared away; but the ſtamina preſerve their ſtrength, and their innate qualities, againſt all theſe accidents; and come out poſſeſſed of that phoſphoric quality, which was before oppreſſed and weakened by the juices. But ſince nature has formed not only theſe plants, but many, if not all the reſt, with the ſame ſort of fibres, I have no doubt but that, by a like proceſs, other plants may be brought to the ſame clearneſs and brightneſs. For ſince the compoſition and mode of mixture are the ſame in all, as chymical reſolutions ſhew us, why ſhould we not attribute to them all the ſame phoſphoric quality? And I have the following experiment to ſupport my conjecture.

I macerated the fibres of the leaves of a garden willow in water, very ſlightly, and making up a maſs of that ſubſtance, and placing it in the day-light, I obſerved upon it a feint and tranſient, but at the ſame time a moſt manifeſt, light. And this reaſon made me obſerve formerly as to vegetables, that they ought not to be ſuppoſed to be otherwiſe than phoſphori.

36. From linen macerated, rubbed, pounded, and reduced to a ſort of liquid pulp, is made paper; a moſt uſeful production, and which wants nothing but ſcarcity to intitle it to the higheſt compliments. This is alſo a phoſphorus: Philoſophers ſhould therefore hold it in great eſteem: I am ſure it is intitled to my acknowledgments, for it opened the way to my diſcovery of this multitude of phoſphori. For when I was looking for ſomething elſe, I accidentally obſerved a piece of paper ſhining with a moſt beautiful light. At firſt I aſcribed it to the whiteneſs of it's colour, and therefore endeavoured to find the like appearances in a great number of other white bodies. But obſerving that ſome of theſe were phoſphori, and others not; and going on in this train of obſervations, I at length arrived at the knowledge of thoſe things which I have already delivered, and am now going to purſue. Paper then, after it has been vexed by this long proceſs, retains that phoſphoric faculty which it received from it's firſt origin, and is therefore a great proof of the ſtrength of it.

Theſe appearances are more wonderful in the paper, than they are in the linen out of which it is made; inaſmuch as the paper is more unlike the ſtamina of the plants from which theſe two curious productions are made; for who can find in paper the leaſt trace of the original form of flax or hemp? That whiteneſs, which is not exceeded by that of any other body; that denſity, which is at the ſame time not altogether impenetrable by wet, or light; that flexibility, which is at the ſame time tenacious of folds; it's ſmoothneſs, it's thinneſs, and all thoſe other qualities that diſtinguiſh it ſo much: what difference is there between theſe and that wet, ſoft texture, that verdure, fragility, and all the other qualities of a tender bark? But if we eſtimate things not by their outward appearance, but by their internal conſtruction, we ſhall find, that human induſtry has made a very ſmall alteration, or perhaps none at all, in the real vegetable nature of the plants, but has only introduced a new diſpoſition of the ſenſible parts. For what do we find in the fibres of the ſtalks, from which linen, and afterwards paper, is made, which theſe two bodies do not, upon an accurate ſcrutiny, diſcover to the induſtrious chymiſt, whoſe art extends to the internal compoſition of bodies? It is well known that paper, and likewiſe the fibres of plants, from whence linen is made, do afford in the proper furnaces, firſt water, then another liquor of an acid nature, next oil; at firſt mild,

afterwards

afterwards acrid and more burnt; laſtly, thick and blackiſh. At the end of the proceſs is left a ſort of coal, chiefly of an earthy nature, black, and poſſeſſing an acrid taſte, from the intermixture of oil and an alkaline ſalt. From whence it plainly appears, that in the whole ſeries of theſe changes, although the external form of the bodies is loſt, yet the inſenſible parts, and what conſtitutes their natural and internal mixture, remain entire.

37. However, whether this phoſphoric quality in the paper be derived from it's vegetable origin, or be occaſioned by it's laborious manufacture, it is yet ſo fine a phoſphorus as could not be paſſed over in ſilence without ſome loſs to natural philoſophers, eſpecially as theſe particulars have not been mentioned by any other writer. Much leſs can I omit what I afterwards learned by experiments made upon paper itſelf. Though paper be, as we obſerved before, a conſiderable phoſphorus, yet, upon the application of a moderate heat, at leaſt ſuch a heat as does not at all hurt it, the paper will become ſo ſplendid as to be in a manner a new phoſphorus. I proved this by two experiments: in the firſt I expoſed the paper to the fire, without the interpoſition of any other ſolid body; in the ſecond I applied the fire by means of a ſolid heated body. The firſt experiment was in this manner: I laid a ſheet of paper upon a gridiron, and placed upon it a ſquare flat tyle ſo as to preſs the paper cloſe to the bars. Underneath

neath were placed burning coals. When the paper was heated enough, the tyle was taken off, and the paper, hot as it was, expoſed to the light, and afterwards removed into the place of obſervation. It was there a very pleaſing ſight to obſerve the paper ſhining with an uncommon light, and to ſee the figure of the gridiron expreſſed by the moſt elegant mixture of ſhade and light. For in thoſe places which were freely expoſed to the action of the fire, there was the brighteſt light; but in the other parts that were covered by the bars, and which had only the uſual quantity of light upon them, there was the appearance of ſomething like darkneſs, which exhibited the outline of the bars determined with the greateſt preciſion. The duration of the light was equal to it's ſtrength; for it continued ten ſeconds at leaſt, ſo lively, that the repreſentation of the gridiron remained very diſtinct; and when the appearances ceaſed, the paper was again expoſed to the light, and being brought into the place of obſervation, the ſame appearances were again obſerved. This being often repeated, the brightneſs of the image went off by degrees, and at length, after being many times expoſed to the light, and taken into the place of obſervation, they at length entirely diſappeared, leaving the uſual light of the paper evenly diffuſed over every part of it.

38. The ſecond experiment was made in the following manner: there was a pretty thick plate of braſs, which was made ſo hot that it could hardly

hardly be held in the hand, but at the ſame time would not hurt the paper. Being placed upon the paper, and continued there till the paper was hot, it was taken away, and the paper removed, as uſual, into the place of obſervation. In the part that had touched the plate, it appeared of an extraordinary brightneſs, and ſhewed the figure of the plate perfectly defined: the reſt was as in the former experiment. Long ago I made another trial of the ſame thing, with a view of determining the duration of the light received from the fire. For this purpoſe, having heated the paper, by means of the plate, in the uſual manner, I expoſed it for a few ſeconds to the light, and preſently drew it back again into the dark. I repeated theſe movements fifty times, and found that I could clearly diſtinguiſh the figure of the plate every time. After that there was left a very ſmall light circumſcribed by the outline of the former figure; and ſoon after the whole appearance was gone off; and the paper, upon being expoſed to the light, produced only that moderate and equable appearance which was natural to it.

39. But I muſt mention one circumſtance, namely, that when the adventitious light began to go off, I ſeveral times expoſed the oppoſite ſide of the paper to the external light; and as often as I did ſo, I obſerved the figure of the plate to appear much brighter on that ſide than on the upper one; and yet the latter had been in contact

with

with the heated plate, and had been often refreſhed by the external light. From whence it may be inferred, that the phoſphoric quality, which is acquired by the application of fire, may in theſe kind of phoſphori be worn out and exhauſted by the repeated application of light: though this does not hold with reſpect to the natural phoſphoric power. This obſervation was made long ago in relation to the Bologna ſtone: as to which it has been often laid down as a rule, that in order to preſerve it for a long time fit for experiments, we ſhould not often produce it in the open and lucid air. But I think it may be proper to enquire, by means of accurate experiments, whether this property is obſervable in the other phoſphori which are made ſo by the appliction of fire: for if it be, we ſhall have another new criterion in both the kinds of phoſphori, that is, the *natural* and *artificial*, which may perhaps aſſiſt us in our further enquiries.

40. Nor is it leſs remarkable that this bright appearance of the plate was very clearly diſtinguiſhable, though there was no heat in the paper perceptible to the touch. And from hence it is eaſy to conclude one of theſe two things, either that the corpuſcles of light, or of that body in which light reſides, continue much longer in bodies where they are fixed than the corpuſcles of heat: or if we attribute an equal power in this reſpect to each of them, then we may conclude that the ſight and the touch do not judge of their reſpective objects with

with equal accuracy: from whence it may follow, that one may be ſtruck, when the other is little or not at all affected.

41. It will not be difficult to propagate this laſt opinion amongſt thoſe philoſophers who maintain that one and the ſame body of fire is both heat and light: that is, it is heat when the fire itſelf is greatly agitated with a variety of motion; and that it is light, if the corpuſcles of the fire are diſpoſed in certain ſeries, and, being driven forward in right lines, ſtrike upon the eye of the obſerver. Theſe gentlemen will judge for themſelves how far my experiments coincide with their opinion. It is certain that they can find nothing more likely to change the heat impreſſed upon the paper into light, according to their notion, than this application of the external light. This light being diſpoſed in right lines, and thrown from the ſhining body by quick and ſtrong vibrations, will impel, direct, and order the congenial matter which it finds upon the paper. By this means, that which only affected the touch, as heat, will, upon being excited by other light, ſtrike upon the eye. In this particular, therefore, they will find theſe experiments very agreeable to their notion. But, I apprehend, they will not be able to reconcile to their opinion that certain and accurate definition of the figure which we have before related. For we do not ſee how that figure could be ſo exactly taken, or ſo firmly retained, by fiery corpuſcles. For they muſt all allow that it is the nature of heat, or fire, to endea-

vour conſtantly to expand itſelf, and in fact to expand itſelf every way. How then can it be, that this reſtleſs and impatient ſubſtance ſhould remain quiet within the outline of the plate? And if it ſhould paſs that, and be diffuſed over the reſt of the paper, and if it ſhould then be expoſed to the external light, would it not become all over a phoſphorus; and either inlarge the image of the plate, or rather confound and obliterate it? Theſe things ſhould be well conſidered by thoſe philoſophers who chuſe to try their parts in this curious inveſtigation. They ſhould further be acquainted, that by the heat of fire, not only paper, but many other bodies, have their phoſphoric light encreaſed: ſome of them retain the ſame degree of light which they had before: others have their natural light ſo far weakened, that it would appear quite extinguiſhed if it did not revive upon removing the adventitious heat. But I have not yet had an opportunity to prepare ſuch a number of obſervations as might enable me to digeſt theſe bodies into certain kinds: ſo that by comparing them together we might learn how far they agree with the nature of fire and heat, or how far they differ from it.

42. But it is now time to treat of thoſe phoſphori, that become ſuch by the operation of fire. And here it muſt be remembered, that ſuch a degree of fire only is required as may affect only the external texture of the bodies, and that in a ſlight degree; and not ſuch as may diſſolve or

deſtroy

deſtroy the internal conſtitution. Therefore let the fire be no more than ſuch as may leave ſome traces of it's operation upon the bodies: and may not change their form, but leave them, in fact and name, ſuch as they were before: for we do not mean to introduce cinders, aſhes, or calx's, but the bodies as they were. Moreover, theſe phoſphori are not to be produced by burning, but only by drying, or roaſting. And in mixed bodies we cannot roaſt the watery humour; for this illudes the force of the fire, and becomes vapour, and returns again to water, if properly received: nor do we roaſt the ſaline or earthy particles, for theſe vitrify, or they dry up, or become calx's: but the *oils* and *fat* parts, which conſtitute the principal parts of the bodies, theſe are the objects of their roaſting. Vegetables and animals abound moſt with theſe parts. And there is great reaſon to ſuppoſe, that almoſt all animals and vegetables, if properly roaſted, may be brought into this claſs of phoſphori. But we muſt truſt to obſervations and not to conjecture. Therefore, in this place, I ſhall relate nothing that is not founded upon experiment; reſerving other conſiderations for further trial.

43. The fleſh of animals, if roaſted, is a phoſphorus. I tried the experiment only upon white meats, ſuch as poultry: for I was afraid to venture upon brown meats, well knowing that the light is rejected or loſt in them. Bones likewiſe, which are natural phoſphori, become brighter,

and of an unuſual colour, by burning. Likewiſe the *nerves*, upon being roaſted or dried by a moderate heat, become very ſplendid and durable phoſphori. And ſince almoſt the whole animal ſyſtem conſiſts of nerves, and ſince all nerves contain a viſcid, tough, and glutinous matter, I did imagine, that if animal glutens were roaſted, they might become good phoſphori: and I found myſelf not miſtaken; for the common glue uſed by joiners, and iſinglaſs, become phoſphori by roaſting. I was not leſs attentive to other animal ſubſtances. I roaſted many of them: ſome ſucceeded, and others did not. Feathers and hoofs, which, as was ſaid before, are naturally dark; and likewiſe the whites of eggs; though I have tried them ſeveral ways, yet I could never make them phoſphori. But it is otherwiſe with the ſhells and the yolks. But theſe laſt do not become phoſphori till they are hardened and reduced to a ſolid and almoſt a dry maſs, by preſſing out the oleaginous part: for whilſt they have that juice, they will grow ſofter, by the application of fire, and not acquire that drineſs which is requiſite to make them bear roaſting without taking fire. But when they are made dry, or at leaſt leaner, by having the oil taken out by means of a very gentle heat, and ſuch an one as does not mark them at all, they then become phoſphori, and produce that light, which the abundance of the oily juice cauſed to adhere to the bodies, or oppreſſed and prevented from

ſhining

ſhining forth. But now, how comes it to paſs that one part of the egg ſhould become a phoſphorus by ſo ſlight a proceſs, and the other is not to be made ſo by any means? How comes it that when Lemery was endeavouring to make Homberg's phoſphorus, out of each part of the egg, he found that the yolk eaſily anſwered his purpoſe, but could do nothing with the white? Theſe are ſtrange effects in theſe bodies: what connection is there between theſe phoſphori of mine, and that of Homberg? Is it not ſtrange that the ſame ſubſtances, which remained dark, or become phoſphoric when expoſed to the light, ſhould, in his experiment, be not at all affected by the contact of the air, or ſhould, on the contrary, be ſet on fire by the air, and blaze? In truth, this is a new field of meditation and enquiry. But I have no time at preſent to enter into it, nor does it fall within the compaſs of this work: for I am now giving an account of experiments, and not debating upon philoſophical queſtions. To return; I muſt obſerve, that in this enquiry concerning animal ſubſtances, I made ſeveral experiments upon milk. For being perſuaded, that white bodies were in a ſingular manner adapted to theſe appearances, I could not help trying experiments upon milk and ſnow, and other very white bodies. But I had no ſucceſs in the firſt two. As to the milk, I aſcribe my ill ſucceſs to its fluidity: and as to the ſnow, I thought that ſome other qualities, which need

not

not now be mentioned, had diſappointed me: for in reaſoning upon abſtruſe and new ſubjects, we are apt to think in a very confuſed manner. However, whether this conjecture about the milk was well or ill founded, I ſtill continued to make experiments upon it. For I could not help thinking, that there were ſome qualities in milk, which, upon being properly managed, would make it become a phoſphorus. I knew, for inſtance, that it contains ſome very viſcid parts, which is a circumſtance well known to many artificers, who make a very ſtrong and uſeful glue out of cheeſe well waſhed and pounded with quick-lime. I therefore conjectured, that cheeſe well dried and toaſted would afford a good phoſphorus: and I found my conjecture to be right.

44. The ſame proceſs, which has ſo much increaſed the number of animal phoſphori, may be applied with the like ſucceſs to vegetables. The gums make the greateſt figure in this reſpect, the gum arabic in particular; after that the gum tragacanth; myrrh likewiſe affords ſome light, though mixed with a dark hue; which laſt may be owing to the marks of the fire, for it is difficult to avoid them. However, the greateſt care muſt be uſed in applying the fire to prevent any mark of burning, and that the bodies may be dried only. It is uſual for the gums to ſwell upon being expoſed to the fire; which is cauſed by the obſtruction which the incloſed humour meets with from the viſcous and tough covering. For

which

which reaſon a very gentle heat muſt be kept up, ſo that the watery parts may be inſenſibly and eaſily evaporated. By this means the body will be left dry, and as it were cartilaginous, and will have it's phoſphoric qualities in proportion to it's whiteneſs. With the ſame attention we ſhould roaſt all kinds of nuts, pulſes, grains, and flour of wheat, if we would have good phoſphori. Theſe bodies in drying, if they be white, ſhould be brought to a very ſlight yellow; or, if they are naturally yellow, as many are, they muſt then be made of a faint red. This obſervation particularly relates to pulſes, which, if dried in this manner, become brittle and eaſily ground. And by theſe marks we may know when bodies are properly prepared. In ſhort, by this ſlight proceſs, I have found very fine phoſphori in almonds, in the fruit of cheſnut trees, beans, vetches, peas, oats, and wheat; in a word, in every kind of grain that I happened to think of: and ſince I have obſerved that all the experiments ſucceeded well, when tried upon all theſe bodies indiſcriminately, I thought proper to give over, for I ſaw there would be no end of the ſubſtances of this kind that are phoſphori. Amongſt other experiments I muſt not omit thoſe that were made upon that grain that is now ſo faſhionable in Europe. I do this the rather, becauſe coffee, which is what is meant, does not belong to any of the claſſes of vegetables before-mentioned, but clearly a different one. For it is not a ſpecies of

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the bean, but, according to *Jeſſicu*, is the ſeed of a tree which belongs to the genus of jeſſamines: this ſeed, therefore, when moderately burnt, and not roaſted as it is for drinking, affords a reddiſh dark light, altogether unlike that which true pulſes afford: by which it clearly appears, that the natural conſtitution of bodies has a great ſhare in cauſing this difference of light.

45. I ſhall juſt mention ſome experiments that have been made upon bread. This body holds a conſiderable rank amongſt theſe phoſphori; nor does it require any other proceſs than that whereby it is made bread out of meal or paſte. For the kneading and the concoction, which make it bread, do likewiſe make it a phoſphorus. It ſhines moſt in thoſe places that are leaſt baked, and inclines more to white than red. The crumb of bread, not toaſted, is dark; but, upon being toaſted at a gentle fire, ſo as to be juſt yellow, it becomes bright, and for the moſt part brighter at the extremities and angles than in other places. Obſerve therefore by how eaſy a change meal, which is naturally dark, becomes phoſphoric. But there is another phoſphorus made from the ſame materials by a much ſlighter change; it is not indeed ſo ſplendid as bread, but it is a manifeſt phoſphorus. It is well known that *wafers* are made out of meal diluted with a great deal of water like a liquid glue, and dried upon hot iron plates. It is uſual to colour them with red, otherwiſe they are perfectly white, which ſhews that

that the meal has ſuffered nothing by the heat of the plates. Theſe wafers are good phoſphori, but that quality is increaſed by heating them, as was before obſerved with reſpect to paper.

46. Hitherto we have treated of ſuch phoſphori as are produced by ſuch a force of fire as ſcarce makes any ſenſible alteration in the bodies. But a greater force of fire muſt be applied, even ſo great, as to diſſolve, and ſet at liberty, one or more of the elements of the body, in order to give this quality to thoſe ſubſtances of which we ſhall now treat. We ſhall mention in the firſt place certain reſins. Theſe muſt firſt part with an acid liquor; and then with that oil, which, on account of it's ſubtility, is called ætherial. After this proceſs there will remain a certain ſolid tranſparent brittle maſs, ſmooth as glaſs, and poſſeſſing the attractive quality of amber. This is called colophonia, upon account of it's likeneſs to the reſin of that name. This is a phoſphorus; but I cannot ſay that all reſins, when treated in this manner, do become phoſphori; ſince I have obſerved it only in the pine reſin, in amber, and in turpentine. But I do not deſpair of bringing many other reſins into this claſs, either by ſome particular proceſs, or perhaps by the proper application of fire. In truth, whenever I have been preparing the above mentioned reſins, I have conſtantly found, that the proper management of the fire was the great point of the work. For there is a certain meaſure in drying and concocting them, of which, if the operation falls ſhort, they

ſtill remain dark: and, if it be exceeded, they loſe the phoſphoric power. Therefore they muſt be treated with ſo gentle a heat as to concoct them; and not ſo great a one as to make them begin to burn. And I have found by experience that this meaſure may be known by the colour of the oil that iſſues out. For it is at firſt white, and when it begins to turn yellow, we may know that we muſt immediately ſtop; or the proceſs will not ſucceed. For this change of the colour ſhews, that the oleaginous part of the reſin is beginning to be too much burnt. And this agrees with what was before obſerved concerning the heating of other bodies: wherein we ſhewed that the *colour* was the rule to go by in the application of the fire. And perhaps, after all, there may be found out ſome one ſingle method for the preparation of all theſe phoſphori: nay, perhaps there is ſome principle in them, upon which the fire acts in the preparation, and the light, after they become phoſphori. Certainly, in mixed bodies, it is the oleaginous part that is principally concerned in bearing the fire whilſt they are roaſted or dried. But they cannot be dried unleſs the moſt ſubtile parts of them be evaporated; much leſs can they be roaſted. Therefore, if we allow that the turpentine reſin, whilſt we are concocting it, and condenſing it into a colophonia by means of a heat not greater than boiling-water, ſends forth an acid liquor, and a thin oil: if this is the caſe, may we not imagine

that

that glutens, leguminous and frumentaceous grains, together with meal, and other bodies of that nature, when roaſted by a heat full as great, do likewiſe ſend forth certain very ſubtile vapours of the ſame kind: and may it not be imagined that the oil which is left in theſe bodies is condenſed, and made ſolid, in the manner of a very thin colophonia? Beſides, other arguments might be produced if there was occaſion, or if it come within the compaſs of this work. But I ſhall reſerve them for another opportunity. For I am in hopes that by ſome proceſs or other, all reſins, and every body of an unctuous nature, may be made phoſphoric. And if I do this, which has in fact already ſucceeded with reſpect to many ſubſtances that are not already phoſphoric, but have become ſuch upon being dried, we ſhall then find that our enquiries upon this ſubject will be much clearer and ſafer.

47. And now we muſt ſpeak of thoſe particulars wherein theſe artificial phoſphori differ from the natural. But we muſt firſt make proper diſtinctions amongſt the artificial phoſphori; otherwiſe we ſhall miſtake for general properties ſuch as relate only to a few of them. Some of them, as we have before obſerved, become phoſphori without the application of fire; as linen and hempen cloths and paper: there are others which have no ſenſible alteration by the fire, as wafers: and a third ſort of bodies become phoſphori in conſequence of ſome change produced by the fire. And this change either affects

the conſtitution of them, lightly and ſuperficially, or it affects their interior diſpoſition, and entirely deſtroys their conſtitution. The former of theſe happens with reſpect to bodies that are roaſted; and the latter is the caſe of ſuch bodies as are burnt, or being firſt diſſolved, in Baldwyn's method, by the force of corroding liquors, are afterwards made very hot. Such bodies therefore as become phoſphori by maceration, or other like cauſe, and not by means of fire, are in every particular the ſame as natural phoſphori. But the phoſphori that are produced by fire have many things quite ſingular. This does not extend to the kind of light to be imbibed; for in this reſpect, and the eaſineſs of being excited, they reſemble natural phoſphori: but theſe particulars conſiſt in the nature of their phoſphoric power, which they derive from the fire, and in the kind of light which they exhibit.

48. With reſpect to the phoſphoric power, we muſt uſe ſome diſtinction in order to explain, and ſet in a proper light, the ſeveral varying circumſtances, which are conſiderable. It muſt be obſerved, in the firſt place, that theſe artificial phoſphori, which are prepared by roaſting, do in time ſo intirely loſe that phoſphoric power which they derived from the fire, as to return to their original darkneſs; this is intirely different from the conſtant and perpetual exiſtence obſerved in the natural phoſphori, which reſiſts all force, and perhaps will ſurvive after the intire diſſolution of their

their fabric. But ſuch bodies as become phoſphoric by burning only, or by Baldwyn's proceſs, ſeem to contain a double phoſphoric power: one of theſe is lively, and in many reſpects like that which is found in roaſted bodies; the other is weaker, and not much unlike what we ſee in natural phoſphori. The former of theſe, which immediately after the proceſs was bright and vivid, does not long continue ſo; but, like the roaſted phoſphori, goes off by ſlow degrees, and is at laſt intirely loſt: but the ſecond kind of light, if we may gueſs at the future by what we have already obſerved, will always laſt. The aſhes, or rather the moſt ſubtile earthy particles that remain in the *lixivium* of burnt plants, hath been found after many years to be very conſiderable phoſphori. I have likewiſe obſerved, that concretions of ſalts, formed ſpontaneouſly in theſe lixivia, by keeping a good while in a warm place, and which are equal to the famous Glauber ſalts; and likewiſe, that the ſalts, which are extracted from aſhes burnt with common ſulphur to a white powder, are very bright phoſphori, after being kept for a great number of years. Therefore the phoſphoric power, ariſing from fire, does in burnt bodies end in a ſmaller degree of power, but ſuch a one as appears likely to be perpetual; whereas in roaſted bodies the power is not only leſſened in a much ſhorter time than in the foregoing, but ſeems to be altogether extinguiſhed. But the duration of it is not the

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ſame in all of them. The gum arabic continued for ſix days at moſt; bread did not laſt one day; and coffee but a few minutes.

49. And as this virtue is ſoon loſt, ſo it may be ſpeedily and eaſily reſtored. It is enough to warm theſe languid, or rather extinct, phoſphori; for there is no occaſion to roaſt them much. By this means the loſt virtue revives. In this reſpect they reſemble the Bologna phoſphorus, and other ſubſtances prepared in the ſame manner. But how often this may be repeated, and the phoſphoric power reſtored, I do not know, for I have not tried it; nor does it appear that it has been tried by any body upon our ſtone, or by Du Fay upon his phoſphori: for which reaſon I hope to be excuſed for the omiſſion of this particular, which is certainly of ſome conſequence; or rather I beg leave to defer it, together with many other particulars, to ſome other opportunity.

50. In the mean time, I muſt mention ſomething which I conſtantly obſerved with reſpect to the phoſphoric power of roaſted ſubſtances. They do not diſcover this quality immediately upon being taken from the fire. You muſt firſt ſuffer ſome part of the heat to go off; the virtue acquired from the fire grows ſtronger as the body grows colder: ſo that heat ſeems to be oppoſite to this quality. In this reſpect likewiſe theſe phoſphori reſemble that of Bologna, which, when hot, does not exert this quality: and even though

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it be not hot, yet, if it be newly prepared, and not uſed to the light, it will perform but indifferently.

51. And theſe are the principal points wherein the phoſphoric power acquired by fire differs from the natural; but the artificial phoſphori, and the natural, differ likewiſe in the appearance of the light itſelf: for the artificial exhibit a brighter light, but which is reddiſh, and ſometimes dark; they likewiſe retain the light ſomewhat longer; but the natural phoſphori have a light that is of a whiter kind, or tinged with a tender yellow, and they loſe their light ſooner. But theſe appearances are not ſo conſtant, nor ſo general, as not to afford a great deal of variety. For there are ſome natural phoſphori that have a very bright and durable light; and by this means firſt occaſion themſelves to be obſerved by the philoſophers, There are others; for inſtance, the lapis lazuli, which afford a reddiſh light; and laſtly, amongſt the artificial phoſphori, the colophonian turpentine, which, according to the nature of this kind of body, produces a light which is rather ſtrong and durable; does likewiſe preſerve it's phoſphoric quality for many years; which in this ſort of body is ſingular. But there are very few, conſidering to what numerous kinds they belong; and, in ſo great a number, no regard is to be paid to the appearance of a few individuals.

52. Since therefore that phoſphoric power which is naturally produced, or at leaſt not by the force of fire, and likewiſe that phoſphorus

which

which is found in antient calxes, are endued with qualities differing from thoſe that are found in roaſted bodies, or bodies newly burnt, it may hence be eaſily ſuſpected, that there are two ſpecies of this power. This conjecture will be ſtrengthened, if it ſhall appear, that each ſpecies is founded upon certain and definite qualities. And, in truth, the phoſphorus in bodies roaſted or newly burnt, ſeems to be fixed in its oily, or as the chymiſts call it, it's ſulphureous principle. For we have already ſhewn, that this principle is more adapted to roaſting than any of it's others; and many bodies newly burnt, make this notion very probable, by the ſulphureous odour exhaled from them. But the phoſphorus of natural bodies, or of bodies that have only undergone *maceration*, or ſuch like change; and likewiſe the phoſphoric power of ancient calxes, if they reſide in any element in particular, may well be ſuppoſed to be fixed in the earthy element. For bodies, which are phoſphori naturally, and without any preparation, do, for the moſt part, upon being burnt, become calxes; and have therefore a great deal of earth and of lime, or rather limiſh matter, in their compoſition. By maceration the fatter parts of mixed bodies are preſſed out; conſequently the earthy parts remain drier, and more ſolid: moſt certainly they are relieved from that matter which principally obſtructs their phoſphoric qualities. Laſtly, in ancient calxes this earth ſeems to remain pure and uncorrupted; and being entirely fixed, and of the moſt unchange-

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able nature, it naturally affords more than any other body, thoſe conſtant and perpetual appearances that are mentioned before. But it is quite otherwiſe with reſpect to ſalts and ſulphur. Salts very eaſily ſuffer damage by the air, by moiſture, and by the prevalence of oppoſite ſalts; ſulphur, on the other hand, very eaſily frees itſelf from any foreign mixture, and is eaſily diſſipated by any ſlight agitation, being naturally a very light ſubſtance. This being the caſe, it is not at all ſtrange that ſulphur does not continue long in bodies: but whilſt it does continue, the phoſphoric power, which adheres to it, and is by that means viſible in freſh calxes, remains entire and unchanged: but when the ſulphur flies off, and is at laſt deſtroyed by time, then the phoſphoric power is alſo by degrees weakened and conſumed.

53. For theſe reaſons it may be eaſily concluded that there are two ſpecies of the phoſphoric quality: and if we give them their names from thoſe bodies in which they are moſt remarkable, we ſhould call one of them the *phoſphoriſm of earthy bodies or lime-ſtone*; and the other the *oleaginous or ſulphureous phoſphoriſm*: following in this reſpect the example of electricity, one ſpecies of which is for a like reaſon called *vitreous*, and the other *reſinous*. However, I have a ſuſpicion that both theſe phoſphoric qualities have one and the ſame principle, wherein they both agree, and which is not very complicated. And if I could ſuffer myſelf to

purſue this train of ſpeculation, I might perhaps imagine that not only theſe forces, which in different ſubſtances ſeem ſo various, but likewiſe others which appear full as different, might all be properly reſolved into one general and common principle; or that at leaſt there is ſome ſecret connection and agreement amongſt all of them. In this I ſhould imitate ſeveral philoſophers of the greateſt ſagacity, who have entertained ſuſpicions concerning a like connection of many properties that appear very different, founded upon reaſons not much ſtronger than mine. What, for inſtance, can have leſs connection with the electric power, or with light, than dew? And yet there are philoſophers who have ſuppoſed, that becauſe, of all bodies, metals only cannot be made electric nor phoſphoric, nor can be wetted by nocturnal vapours of a ſerene air; that for theſe reaſons the three accidents above named either have one common nature, or at leaſt ſome connection not yet diſcovered. And why ſhould not this ſuſpicion appear probable to me, who have ſeen by experiment that metals cannot be made phoſphoric? But we muſt leave theſe conjectures: eſpecially ſince I have ſome hopes that at ſome time or other they may be made phoſphoric. Perhaps this may be obtained by ſome common and obvious preparation; which for that very reaſon has never yet been thought of. And, indeed, I ſuſpect that the preparation of Helmont's flint, mentioned in the beginning, remains at this day unknown, for that

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reaſon only: for who would imagine that by burning only, that ſtone might be made a phoſphorus, which bears the heat of the fire, ſo as to vitrify, rather than be turned into a true calx? And yet a ſtone of this ſort may be burned in the potters furnaces till it becomes white and brittle, and ſo a good phoſphorus, as to catch the light very eagerly, and keep it for a long time. And that no one may be miſled by that appearance of brittleneſs and whiteneſs, it is proper to inform them, that this ſtone, ſo burned, cannot be diſſolved by liquids, or acted upon by corroding liquors: by which it plainly appears, as was juſt now ſaid, that it differs very much from the nature of a true calx. Therefore perhaps metals, by ſome eaſy proceſs which may at this time be not unknown, may have thoſe qualities which more laborious proceſſes have not yet produced. I have obſerved that the *ſalt* which is a concretion of *cerus*, diſſolved in ſpirit of vinegar, and is uſually called ſalt, or ſugar of *ſaturn*, is a weak phoſphorus, but a very viſible one: but for this purpoſe it muſt be kept dry, and not be ſo old as to have loſt it's tranſparency. To this claſs we muſt refer that orey ſubſtance which is found in ſmiths furnaces, and is commonly called tutty. This body is uſually ranked amongſt metallic ſubſtances, and very properly; for it ariſes from the vapour which many minerals and ſome metals throw out when they are fuſed. Theſe vapours flying upwards, adhere to iron rods fixed at the top of the furnaces:

covering them with incruſtations of an aſh colour, and ſometimes of an azure colour: theſe incruſtations are like the barks of trees, ſmooth and hollow on the inſide, but on the outſide rough and granulated, and with a convex ſurface: ſo that by the form it is plainly ſeen that they have grown to oblong and round bodies. The tutty therefore, when expoſed to the light, is a ſtrong, red, and laſting phoſphorus. But what is very wonderful, this quality is confined only to the convex part, which, as we ſaid before, is rough, and does not extend to the concave part, which is ſmooth, or at leaſt not altogether unpoliſhed. I do not mention in this place other refuſe of metals, ſuch as the flowers, as they are called, and the calxes: which, though they have ſome phoſphoric appearances, yet it is uncertain whether theſe are to be aſcribed to the metallic parts, or to ſuch earthy and ſaline mixtures, as theſe concretions are by every body ſuppoſed to contain.

54. I ſhall therefore for the preſent lay aſide all thoughts of making metals phoſphori, and ſhall apply myſelf to conſider that property of the tutty which was juſt now mentioned, namely, that it's rough ſurface is a bright phoſphorus, whilſt the ſmooth and hollow ſurface is for the moſt part dark. This circumſtance would not be very material, if it did not fall in with other obſervations; but, where that happens, it is not ſafe to overlook any thing. I have ſeen ſome ſpecimens of marble that would not be phoſphori, or

at leaſt would throw off the light to the extremities ſo long as they continued poliſhed; but, upon loſing the brightneſs of the poliſh, became good phoſphori. If therefore we may have leave to venture a conjecture, we may ſay, that ſurfaces are either phoſphoric or not, according to their diſpoſion to reflect the light. And if this be the caſe with reſpect to *reflection*, why ſhould it not with reſpect to *refraction* alſo? the reaſon ſeems the ſame, but the queſtion muſt be determined by experiments: I intend to make many for this purpoſe; but, as I have not at this time proper opportunity, I muſt content myſelf with relating one or two that ſeem relative to the preſent purpoſe. I expoſed a glaſs bottle full of pump water to the light, and then removed it into the place of obſervation. I found it, as I expected, quite dark. I then put in a few drops of *oil of tartar*. The water immediately became turbid, and of a whitiſh colour; for this is the nature of ſpring water which abounds with chalky earth. It was again brought into the place of obſervation. Appearances were then changed; for it exhibited a pale light, ſufficient to diſtinguiſh the form of the bottle. Inſtead of the ſpring water was ſubſtituted rain water, wherein ſome lapis ſpecularis had been diſſolved: it is the property of this ſtone, that, being powdered and thrown into water, it diſſolves like ſalt, and does not hinder the tranſparency of the water. When the water was ſaturated with this powder, a quantity of *oil of tartar*

was

was again put in, and the event of the experiment was the ſame. Thus we ſee, that earthy corpuſcles, whilſt they continue very ſmall, diſſipated, and perfectly tranſparent, and whilſt their ſurfaces perfectly coincide with the water wherein they float, do eaſily receive the external light, and it as eaſily paſſes through them; they cannot therefore exhibit in the dark that light which they do not retain. But when the corpuſcles, by the infuſion of the ſalt, are driven together in *thicker maſſes*, and are diſturbed by their mutual contact; in this caſe the light does not find ſo free a paſſage through them, but is obliged to ſtop. In the mean time, whilſt the light continues in the bodies, it remains viſible, provided there be no obſtruction. There are ſeveral circumſtances that may obſtruct, and amongſt theſe are colours and metallic mixtures; for if, inſtead of oil of tartar, you throw in *ſal ſaturni*, you will find that the water is become turbid and milky, but not phoſphoric.

55. What has been ſaid concerning the light, namely, that it appears, or does not appear, according as it is reflected or refracted, has indeed a great appearance of truth: but we are prevented from pronouncing the certainty of it with any great confidence, becauſe it depends but upon a few obſervations; and there are many that ſeem to oppoſe it. Gems, cryſtals, and glaſs, always remain dark, whether they continue whole, or be ground into the fineſt powder. Therefore, theſe bodies

bodies do not derive any phoſphoric quality, either from their tranſparency, or from their extreme whiteneſs: by the firſt of which properties, the light paſſes through them with the greateſt eaſe, and by the other they partly retain the light, and partly reflect it in all directions. In like manner ſome of the cleareſt diamonds are ſplendid phoſphori, whilſt others of them are intirely dark. Laſtly, a great number of opaque bodies, either rough, or poliſhed, or ground to powder, are phoſphori, or the contrary. Which makes it manifeſt, that the ſtructure of bodies, both external and internal, does ſometimes aſſiſt their phoſphoric qualities, and ſometimes the contrary: but it has no certain and conſtant connection with the cauſe of their phoſphoriſm. And if ever we might hope to diſcover this cauſe, the preſent ſeems to be the favourable opportunity. For ſince, by the induſtry of myſelf, and many others, it has been found out, that almoſt all bodies poſſeſs that power, which was heretofore believed to be peculiar to one only, and afterwards to a few: it may eaſily happen, that in ſo great a number, and in ſuch a variety of ſpecies, ſome one may diſcover that ſecret which has been kept ſo faithfully by a ſmaller number. And if we ſhould ever come to a ſolution of that queſtion which has been often debated in our academy, namely, by what means luminous bodies communicate their ſplendor to phoſphori, I do not ſee what bounds can be ſet to the ſpeculations

of philoſophers concerning *light*; for if we ſhould determine that the light paſſes from the luminous body into the phoſphori, which is the opinion of ſome, and which agrees with many forms of expreſſion uſed in this book, we ſhall, in this caſe, have ſomething new and wonderful with reſpect to the light. It is an old opinion, that light conſiſts of exceeding ſmall and active corpuſcles, which are thrown off continually, every way, with a vibratory motion from the luminous body, and with the greateſt velocity: but to imagine that theſe corpuſcles are not preſently diſperſed and diſſolved by the agitation of their own motion: to imagine that they do not immediately vaniſh: and further, that they exiſt when out of the ſources from whence they ſprung, and that for a conſiderable time: and moreover, that they adhere to bodies, which they meet with in the manner of heat and odors: and, laſtly, that they repoſe themſelves in theſe bodies, in a manner inconſiſtent with their ſuppoſed activity: all theſe are new ideas; at leaſt, they are not perfectly underſtood, and certainly have not as yet been clearly explained. If we ſuppoſe therefore, that the corpuſcles of light are not diſſolved immediately upon their leaving the luminous body, but that they continue for a time, and that they are found in the greateſt numbers round about their original ſource: what more can be required in order to conſtitute that atmoſphere about every luminous body which has been aſcribed to the

greateſt

greateſt luminary by aſtronomers and philoſophers? And if theſe atmoſpheres do in a great meaſure conſiſt of *light*, we will not then have any further diſpute about them: for the diſputes in the laſt age, with reſpect to the ſun's light, namely, what was the cauſe of it, were very idle. For this would be quite as uſeleſs as to enquire what makes light ſhine. Thus much with reſpect to the light which proceeds from the luminous bodies. But, if we turn our thoughts to the bodies that imbibe it, is it not ſtrange, that this phoſphoric quality ſhould be ſo common as to be reckoned amongſt their general properties? for who could imagine or conceive what, and how far, and in what manner, theſe moſt ſubtile and active corpuſcles act upon the denſer fabric of thoſe bodies which they penetrate, into which they inſinuate themſelves, and wherein they are retained? But how long are they retained? if we conſult our ſenſes, but a very little time; but, if our ſight could diſcern the very ſmalleſt particle of light, which perhaps ſome animals that ſee in the dark are able to do; in that caſe I believe we ſhould be of a different opinion. For as it is a common notion, and a very probable one, that there is no body truly cold; ſo it may be ſaid, with equal probability, that no body is entirely dark. But our obſervations for this purpoſe muſt be made in a place that never ſaw the light; and conſequently on this earth, which alternately enjoys light and ſhade, no ſuch dark

place can be found. — But if phoſphori do not ſhine by a borrowed light, but by their own, and that their own light is produced only when put in motion, and lighted up by the rays of a luminous body, which opinion is likewiſe confirmed by ſome experiments, we have then new doctrines never before thought of, and a great fund added to natural philoſophy. There will then be a certain hidden and ſecret principle in bodies inflammable by this moſt ſubtile fire. We ſhall then diſcover in nature a perpetual fire, or at leaſt that bodies are very frequently taking fire upon being expoſed to the light. This motion will deſerve a conſiderable rank, amongſt thoſe hitherto imagined by the philoſophers, to be ſerviceable in exciting and reſtoring the forces of natural cauſes. There will likewiſe be a perpetual diſſipation of the matter which is thus ſet on fire; and ſince this happens without any ſenſible detriment to the bodies that are burnt, who can comprehend either the ſubtility of this matter, or the means of reſtoring it, which are adapted to ſupport ſo continual a waſte? But this is not a time to purſue this enquiry, or any of it's conſequences, even in the ſlighteſt manner; for they require more diſquiſition and thought than at preſent can be found. It is enough for me if my obſervations, or ſpeculations, have opened the way to others who may purſue theſe great points.

A FURTHER

A FURTHER

ACCOUNT

OF A GREAT NUMBER OF

PHOSPHORI,

LATELY DISCOVERED BY

J. B. BECCARII, Profeſſor of Philoſophy.

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1. IT uſually happens, that in the inveſtigation of new ſubjects, many things at firſt paſs unobſerved, which, when we proceed further, we are ſurprized have not been taken notice of. I am ſenſible of the truth of this obſervation, with reſpect to my own enquiries, concerning the phoſphoric qualities of bodies. For although I uſed as great diligence as I thought was poſſible, yet I was obliged to leave many things which my obſervations did not reach, or which I had not time

to perfect. And yet these matters required no laborious process, nor any singular degree of industry, but only an attention to some few things, and those apparently of no great moment Moreover these circumstances did not occur to me until I had published the former part of this work. They came into my mind spontaneously, at least without any labour: insomuch that I wondered, and took it hard, and indeed was almost angry with myself, when I reflected that I had not before turned my attention, which is generally ready enough upon those subjects, to these resources, which now appeared to be quite at hand, and obvious.

2. Being prepared, therefore, by a more accurate method of observing, I once more applied myself to my former studies, which I have continued for several months past, and to this day. For it has always been my opinion that philosophical enquiries can seldom be neglected without a considerable loss to the student, and never without some detriment to natural knowlege. For, as on the one hand, it is much easier to pursue a work already begun, than to take it up again when intermitted: so on the other it may be said, that if the first undertaker of any study does not bring it to perfection, it will be long before any other will take it up, which perhaps may never happen. I hope therefore to be excused for returning to my phosphori, after having left them for more than two years: and the rather, because this intermission has been compensated by the addition of an incredible number

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of new phoſphori to thoſe mentioned in the former work. For I can now affirm, without arrogance, that by this induſtry of mine the dominion of light is extended to all known bodies with a very few exceptions. In the former part I declared this new and ſingular property of light, or of the nature of bodies in general, with a great degree of doubt and caution: ſuch as was proper, conſidering that I then could propoſe no more than a conjecture or an opinion. Which conjecture, though ſupported by a very great number of bodies that were phoſphori, was yet made doubtful by a good number of bodies being otherwiſe. But there is now no room for doubt, or fear, ſince we find, by repeated and clear obſervations, that theſe very bodies are in fact phoſphori. I ſhall now deliver theſe obſervations in proper order, and rather fully: ſo as to deſerve the confidence of my readers, and to open to others a ſafe and eaſy way to the ſame obſervations.

3. Since therefore this diſcourſe relates only to thoſe phoſphori, which become ſuch by being expoſed to the light, the ſame rules that were laid down for this purpoſe, in the former work, muſt be obſerved now. For whatever was ſaid there related only to theſe kind of phoſphori, and the whole apparatus for obſervation related to them only, &c.

4. Having diſcovered the ſource of this miſtake, I found it neceſſary to alter my apparatus. Therefore I fixed to the upper margin of the window,

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two curtains of a thick black cloth: one on the inſide, the other on the out: ſo broad as to exceed the ſize of the window at leaſt ſix fingers breadth. Hanging therefore downwards in a looſe manner, and ſticking pretty cloſe to the edges of the window, they effectually excluded the light. In the mean time, the obſerver lifting up the lower edges of the curtains a ſmall matter, was enabled, taking any ſubſtance in his hand, to hold it out in the light at arms length, and taking it in again to examine it. But, in doing this, he muſt ſhut his eyes to prevent their being affected by the light. Drawing back his hand, and upon the curtains falling down, which two operations would take up but a moment of time, and being ſecured from the light by the window being cloſed, he might immediately caſt his eyes upon the body, and judge of it's ſplendor or obſcurity.

5. And in this manner I provided againſt the principal inconveniencies that occurred in my former enquiries. But I found by experience, that there were ſome other particulars to be obſerved, the neglect of which would render any judgment concerning theſe phoſphori fallacious, if not impoſſible. And in the firſt place it muſt be obſerved, that the bodies ſhould be placed in the *cleareſt light*. Now perhaps ſome people may think this a very needleſs caution. For who will not imagine, that the bodies exhibit a light, bright in proportion to the brightneſs of the light imbibed?

imbibed? And yet ſome will be of a different opinion; eſpecially thoſe who imagine, that ſome phoſphori ſhine brighter in a cloudy day than in a clear one, or even than when expoſed to the rays of the ſun. Nor is this notion deſtitute of probable reaſons. Upon which account I think it proper to declare, that I do not find that obſervation to hold true with reſpect to my phoſphori, although it may be true with regard to the Bologna ſtone, which is the principal ſubject to which that obſervation is applied. And if we admit that theſe bodies differ in their phoſphoric quality from that ſtone, ſuch difference may be properly aſcribed to their being of a different nature. Our phoſphori are naturally ſuch, whereas the Bologna ſtone, and other ſubſtances of a like nature, derive that quality from art, and the application of violent heat. As to theſe *natural phoſphori*, I could never find that the *light of the ſun* was at all prejudicial to them. So far from it, that many of them are of a nature ſo difficult, as to perceive the leaſt differences in the light of the ſun; and will not ſhine, unleſs they be expoſed to a light quite clear from every degree of *vapour*. There are likewiſe ſeveral that will not ſhine even by the brigheſt ſun-ſhine; but continue dark until they are expoſed to the rays of the ſun collected by a lens.

6. It is material not only to conſider in what light the bodies muſt be placed, but likewiſe in what ſituation. For there are ſome that ſhine better

better in the ſame light when placed in one poſition than when in another. But it is difficult to lay down any certain and general rule in this matter; becauſe the ſurface and the figure of the body, and the different nature of the component parts, and perhaps many other circumſtances, may make conſiderable differences. It is difficult to foreſee theſe conſequences ſeparately; but to provide againſt all of them together is quite impoſſible. In general, however, that poſition is the beſt wherein the greateſt quantity of light is tranſmitted from the luminous body to the phoſphorus. More light will fall upon a rough ſurface than upon a poliſhed one; for, according to the laws of optics, the latter will reflect a great part of the approaching light before it touches the body. Likewiſe, if the ſurface be uneven by hollows and riſings, care muſt be taken, as far as poſſible, that every part be equally enlightened. For it often happens that the riſings project a ſhade upon the lower parts, ſo as great part of the body will remain dark, even when expoſed to the cleareſt light. For which reaſon, in theſe circumſtances, the whole body will be in appearance dark, eſpecially if it be a weak phoſphorus. For a faint light, if it be broken and diſperſed, will ſcarcely ſtrike the obſerver's eye, and will rather exhibit an appearance of darkneſs than of light. —It likewiſe often happens, that experiments are made upon ſubſtances, whoſe parts differ very much from each other in nature and qualities:

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in which caſe it is evident, that the moſt phoſphoric parts ſhould be turned to the light; that is, the white rather than the dark, the dry rather than the wet, and the opaque rather than the tranſparent: but becauſe it ſometimes happens, that theſe differences are not known at the time of making the experiment, it will be proper to keep changing the body, during the experiment, into different ſituations, in order to diſcover the moſt phoſphoric part of it. This rule is frequently uſeful with reſpect even to bodies that ſeem to be of an uniform nature: I experienced this in a drinking glaſs of a conical figure, which was expoſed to the light, in ſuch a manner, as to have it's axis parallel to the horizon, and in that ſituation exhibited a tract of light, which was likewiſe parallel to the horizon; though in every other poſition the glaſs remained quite dark.

7. It is proper likewiſe, that the obſerver ſhould be very exact in turning the enlightened parts to his eye, and not the dark ones. It may ſeem very eaſy to be exact in this point, but in experience it will be found much otherwiſe: for the hands, which in our uſual motions are directed by the ſight, when they are deprived of that aſſiſtance in this great darkneſs, are very often deceived.

8. But there are other accidents whereby the ſituation of the body is ſo far changed, that the enlightened parts are not expoſed to the obſerver's eye. This happens with reſpect to bodies that

are broken into ſmall pieces, or powdered. Theſe cannot be brought into the place of obſervation, eſpecially if a quick motion is uſed, without being ſhaked. In which caſe it happens, that the faces of many particles are turned round, and there is a confuſion amongſt them: ſo that the enlightened particles loſe all their ſplendor, being covered by the dark ones. For the ſame reaſon it is very difficult to make experiments upon liquids. Theſe muſt be confined ſo as to prevent them from flowing as much as poſſible. This may be done by putting them in bottles of the cleareſt glaſs, which being quite filled, will keep the liquor from being ſhaken or agitated. The light, which paſſes through the glaſs, will be ſufficient to excite the phoſphoric quality in the liquid, provided the glaſs be clear and thin: and when the light is once imbibed by the liquor, enough of it will remain for obſervation, as there will be no motion of the parts. Powders muſt be placed in very open veſſels, and then compreſſed in ſuch a manner as to make the particles cohere. By this means they will preſerve that poſition wherein they received the light, and will exhibit the ſame light entire to the obſerver.

9. Laſtly, it muſt be obſerved, that it is better to make experiments upon large bodies than upon ſmall ones: and if the light be faint, it is abſolutely neceſſary that the body ſhould be large. For it is plain, that a faint light, when diffuſed over a large ſurface, may ſtrike the eye, but not ſo when

when the ſurface is ſmall. Therefore where there can be a choice, it is always beſt to prefer large maſſes. And in ſeveral experiments where I have not been able to find any light at all upon ſmall pieces of bodies, I have yet found that larger pieces were ſtrong phoſphori.

10. But we have not always an opportunity of trying large bodies, either becauſe there are no large ones of the kind, as in the caſe of gems, or becauſe large ones are not to be procured. We muſt in that caſe procure that magnitude by artificial means which nature or opportunity have denied: either by faſtening together many minute corpuſcles of the ſame body, and diſpoſing them in as large a ſurface as poſſible, upon a ſtand of black wax; or, if we cannot procure a number, we muſt powder ſuch as we have very fine, and ſtrew the duſt upon a like ſtand. By this means I have obſerved the phoſphoric power of ſome exceeding ſmall gems, which, when viewed ſeparately, appeared dark: and I have no doubt, but that thoſe diamonds, in which no light has yet been diſcovered, would become phoſphori, if treated in that manner.

11. And theſe are the chief points which muſt be attended to by ſuch as chuſe to repeat theſe experiments; in relating of which, though I may have been ſomewhat long, yet I have ſaid nothing ſuperfluous, unleſs an accurate deſcription of the proper means for conducting philoſophical en-

quiries ſhould be called ſuperfluous. For ignorance of the particulars juſt now related has concealed even to this time theſe extraordinary phoſphoric appearances, not only from very ſagacious obſervers, but almoſt from myſelf, who not only ſuſpected the truth, but was labouring this particular point with the cloſeſt attention. And in truth a vaſt number of bodies have been pronounced by me to be dark, which by this more accurate proceeding I have found to be phoſphoric. And now I ſhall proceed to enumerate theſe bodies: in which undertaking I hope not to be again accuſed of ſuperfluity, as if this particular enumeration could give no light to the general aſſertion in the beginning of this paper: namely, that *all bodies*, except a very few, were phoſphori. For although this quality be common to all bodies, yet each kind of body has ſome ſingularity in this reſpect, which is worthy of notice. And the obſervation of theſe ſingularities gives a probability to the general aſſertion.

12. In this new catalogue we ſhall follow the ſame order that was obſerved in the former work, and ſhall divide our new phoſphori into three claſſes, of *foſſils*, *vegetables*, and *animals*. In that work we gave the firſt rank amongſt the foſſils to *earths*, of which we ſaid, that a great many were phoſphoric, but that ſome were dark. It was obſerved too, that though no certain criterion of their ſplendor, or their darkneſs, could be derived from their qualities, eſpecially from their colour,

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yet I added, that of all colours, white, and the colours approaching to it, were the moſt phoſphoric. But now, ſince my late obſervations, I affirm that all earths are phoſphoric, without excepting even the black or the red. For I have ſeen undoubted phoſphoric appearances, though faint ones, in thoſe black earths uſed by our painters: eſpecially at the *edges and angles*, which laſt is a circumſtance common to many bodies. The *vicetina fullonia*, which was mentioned in the former work, afforded a brighter appearance, though it's light was far from being proportionable to it's whiteneſs: ſo that it is not ſtrange that in giving inſtances of white bodies that were not phoſphori, I ſhould in the former work mention this ſubſtance. For even at this time I was very near pronouncing the yellow earth, which our painters procure from the Campania Romana, to be no phoſphorus: ſo obſtinate was it in refuſing even the ſun's light. But at laſt it exhibited phoſphoric appearances, upon receiving the ſun's rays through a lens.

13. With reſpect to ſands, which I placed next to the earths, I have ſcarce any thing to add to the former obſervations. For I aſcribed to moſt of them theſe qualities, provided the colour did not obſtruct. I aſſerted in the former work, that the ocre which adheres to the yellow ſands, that are found in the hills near this city, hinder their phoſphoric appearances: but I now find that theſe ſands are phoſphori, even though the ocre be not waſhed

waſhed off. I was likewiſe miſtaken in aſſerting that thoſe ſpangles of a golden colour, and which partake of the nature of talcs, and which are mixed with the ſame ſand, were dark: for I find them to be phoſphori.

14. Amongſt the larger ſtones, marbles deſerve the principal place, ſince no ſtones are larger; and beſides, marbles exceed all, in their poliſh and brightneſs. They likewiſe excel as phoſphori. The ſofter and the whiter marbles ſhine the moſt. But the hard and the dark ones are not entirely dark, though I formerly doubted upon this point. I find that porphyry, ophites, and granate, become phoſphori by the ſun's light, but more ſo by the application of a lens: though in the former work I mentioned theſe as uncertain. Beſides the baſalties, a kind of Æthiopian marble, which in hardneſs and colour reſembles iron, may be made to ſhine by means of a lens. To theſe may be added a kind of Ægyptian green marble, and likewiſe the antique green marble, together with the oriental green marble, called della ſtella: though, when I wrote the former part, I thought theſe marbles entirely dark, or at leaſt that they gave a very doubtful light, and that at the *edges only*.

15. In the former part we diſtributed the ſtones of ſmall bulk into their genera, and deſcribed one claſs of them as being not ſo hard as marble, and the other as being harder than any marble. We then ſubdivided the firſt of theſe claſſes into three new diviſions. I have nothing to add to the firſt diviſion,

diviſion, namely, that which comprehends the ſtones which are altogether deſtitute of any regular form. In the ſecond diviſion we place thoſe ſtones which contained a certain regular internal formation with an irregular outſide: and gave for inſtances the amiantus, ſeveral ſpecies of talc, and a certain ætites, from Nani: all theſe bodies were there called obſcure. But later obſervations have diſcovered a manifeſt light, not only in theſe but in others of the ſame claſs: eſpecially in a certain very hard oriental ætites, and even in the paper made from the amiantus. The laſt claſs, conſiſting of ſmall and ſoft ſtones, having regular formations, both internal and external, I, at that time, conſidered as abounding with phoſphori. But having then ſpoken of certain ſpeckled and ſtarlike (ſtones) rather doubtfully, I take this opportunity of declaring them to be very manifeſt phoſphori.

16. Next follow ſuch ſtones as are harder than marble: I found few of theſe that, upon the firſt trial, would ſhine in the dark: certainly much fewer than their brilliancy, when expoſed to the day-light, ſeems to promiſe. But upon further trial I found that they were all phoſphori; and would ſhine if properly aſſiſted. The aſſiſtance of a burning-glaſs was found the moſt certain and efficacious; but ſeveral, eſpecially ſuch as conſiſted of minute corpuſcles, required to have many fragments of the ſame body joined together: or to have their powder, or ſmall duſt,

ſpread

ſpread upon black wax, ſo as to make a *large ſurface* in the manner before-mentioned. In the former work I divided this kind of ſtones into the opaque, the tranſparent, and thoſe of a middle nature: and going through all theſe diviſions, I, at that time, of the opaque ſtones, admitted only the lapis lazuli into the number of phoſphori, excluding all the reſt, and particularly the whole family of jaſpers, and the malachites. But I now find, that all theſe ſtones are phoſphori. For the malachites, and likewiſe the lapis nephriticus, are manifeſt phoſphori: and that jaſpers, whether they be red, or of a dark green, or be mixed with many different colours, ſhine when expoſed to the lens; and many of them by the light only of the ſun, eſpecially ſuch of them as are ſtreaked with white veins.

17. In the other claſs of precious ſtones, namely, that between the opaque and the tranſparent ones, upon examining one hundred agates, I found only one which could not be made to ſhine by the lens. This ſtone was of many dark colours, ſo that I aſcribed it's obſcurity rather to the colour, than to the nature of the ſtone. Moreover, I examined in this claſs opals, onyxes, and carncoli, which I had omitted in the former trials: and found them all to ſhine by the ſimple light of the ſun.

18. The laſt claſs containing cryſtals, and all the tranſparent gems, was much more favourable to theſe experiments than I expected. They had formerly

formerly withſtood many attempts of Du Fay, and of my own, though I have been generally pretty lucky upon ſuch occaſions. But I accompliſhed in theſe laſt experiments what I could not in the firſt. Many of the cryſtals ſhone by the ſun's rays only, and ſome of the more difficult ſhone by means of a lens. But all of them were made to ſhine by means of the day-light only, after they had undergone an eaſy proceſs. I rubbed off ſome very fine and white powder, by ſtriking them one againſt another. This powder they rub upon each other, eſpecially in thoſe places that are rough and unpoliſhed. In conſequence of this, the cryſtals become phoſphori in the places ſo rubbed. The ſame proceſs ſucceeded likewiſe with glaſs, by which means this ſubſtance, which before was thought obſcure, became phoſphoric. With reſpect to gems, they all ſhine when expoſed to the lens, except a very few. The eaſieſt of all are the emeralds, many of which ſhine by the ſimple light of the ſun. After the emeralds come the ſapphires, eſpecially thoſe of the whiter kind: the cryſolites are almoſt as good as theſe: then follow the amethiſts and topazes; next the rubies, eſpecially the paler ones; and laſtly the granates and hyacinths. The laſt mentioned would ſcarce ſhine at all when examined ſeparately, but were much eaſier when taken in numbers; but in all ſituations they are the moſt difficult of gems; inſomuch, that having taken ſome granates of a fine colour, high

poliſh, and great ſize, together with a certain gem of the hardneſs and colour of a ruby, and likewiſe two beautiful hyacinths; I could not make any of theſe bodies ſhine when examined ſeparately. But I aſcribe this, and I think juſtly, to the nature of the colour; for I have found, by a good many obſervations, that red, amongſt other colours, is very oppoſite to all phoſphoric appearances.

19. After the gems, I conſidered metals, following that diviſion which *Woodward* had formerly made amongſt the foſſils. Du Fay formerly tried many experiments upon metals, and I have made not a few. Indeed I have never deſiſted from trying every means I could deviſe to make them phoſphoric, but never with ſucceſs. It is ſurely a hard and ſtubborn kind of body, ſince it rejects light and electricity, and even dew. Theſe metals conſtitute that ſmall number of bodies which appear to me to be the only ones in nature that are not phoſphori. For the darkneſs of all others may be properly aſcribed to ſome accidental and foreign cauſe, and not to their nature. But in metals there is no ſuch cauſe yet diſcovered, to which we can attribute their darkneſs.

20. Whatever has any affinity to metals, I formerly thought, partook of the ſame obſcurity: and I find this obſervation true with reſpect to cinnabar, zink, and mundick; but it does not hold with reſpect to the lapis calaminaris, which I now find to be phoſphori, as are likewiſe ſome other foſſils

foſſils of the ſame kind. For inſtance, I find that ſome loadſtones exhibit a very faint light, which is diffuſed through their whole ſubſtance: and likewiſe, that a certain mineral iron exhibits a ſcattered light. Magnetia is a phoſphorus: ſo is the ſmiris, the bloodſtone, and the armenus, eſpecially with a lens: I had another ſtone of the colour and weight of iron, which appeared phoſphoric by the light of the ſun only, at the ſides which were uneven. Therefore theſe bodies, and a good many others which are not named here, though they have a mixture of metal in them, and though their colour is black or dark, yet they are found to exhibit a light, which, though very weak, may yet be perceived by a very nice obſerver.

21. Following Woodward's method, I aſſigned to earthy juices the laſt place in the claſs of foſſils: and, ſtill following the ſame author, I divided them into ſalt juices and fat ones; and admitted almoſt all ſalts into the number of phoſphori; excluding thoſe only, which had ſome mixture of metal in them: to which I was induced principally by an experiment made upon vitriol. For I found, that every kind of vitriol, however tranſparent, was always dark; but afterwards, upon repeating the experiment more accurately, I diſcovered ſome traces of a faint light upon a cryſtalline lump of this ſort. This light was more diffuſed, but ſtill as faint, in ſome vitriol, that was powdered and preſſed down upon

a plain ſurface: but the light was pretty conſiderable in that white calx into which vitriol is reduced, when expoſed to a hot ſun. By the application of a lens, the light, which appeared in the powdered vitriol weak, and almoſt uncertain, was made quite manifeſt, and clear of doubt; and, by means of the lens, the light in the vitriolous calx was increaſed three-fold.

22. In the former work I was very much miſtaken in regard to the fat juices: for I did not admit one of them to be phoſphori: but at preſent I ſhall do them more juſtice. I have found, that at leaſt amber, and likewiſe ſulphur, as well native as fuſed, are phoſphori. But jet and pit-coal are not. But theſe bodies are ſo black as to caſt a darkneſs upon the brighteſt body: and therefore it is not to be wondered at, that in the greateſt light they have no luminous appearance.

23. Thus far of foſſils. We ſhall now ſpeak of vegetables. In my firſt experiments I imagined that plants were the leaſt phoſphoric of bodies: not that they were without this property, but that it wanted the proper force and vigour: inaſmuch as it was oppreſſed and overcome by being connected with ſome principle that was oppoſite to phoſphoriſm. And it was eaſy for me to frame a conjecture as to this principle, from the experiments which I made upon the wood and fibres, whereof all plants conſiſt; as well as upon artificial phoſphori, which are in a great meaſure made out

of

of vegetables. For wood and fibres being of a drier compoſition than other parts of plants, are therefore more phoſphoric: the whole preparation of ſuch phoſphori conſiſting only in drying them properly: from whence it was eaſy to conceive that plants have an innate phoſphoric quality, which is oppreſſed by the redundancy of moiſture wherewith they abound; and which quality would exert itſelf upon the moiſture being removed. But I am not fond of conjectures, though experiments almoſt oblige me to give way to them: and, I muſt ſay, that I can find nothing in plants which may not be made phoſphoric, provided all the humidity be exhaled. In moſt plants nothing more is required than to be dried in the wind: others will not ſhine unleſs they be firſt heated. In this number are ſome plants of the bull-ruſh kind, and amongſt theſe the tipha. But they all have their ſplendor improved by the application of fire, ſo that it be very moderate, and not to mark them. I could find but one plant which would not ſhine, even when fire was applied, and that is the red beet.

24. But this quality is not ſo entirely ſubdued by humidity as not to exert itſelf in ſome meaſure, even in plants that are in full verdure, and full of juice. Moſt of them will ſhine by a very clear ſun-ſhine: but the obſerver muſt be prepared in the manner mentioned in the former part of this diſcourſe. In that ſituation he will find that almoſt all roots ſhine, provided the colour does not obſtruct.

ſtruct. But the principal ſplendor will be in the ſkin; for the middle will appear dark if it conſiſts of a ſoftiſh pulp. But it will ſhine if the juice be ſqueezed out. Stalks and barks follow partly the nature of the wood, and partly the nature of the boughs, according as their texture inclines to one more than the other. But leaves for the moſt part become ſplendid by the ſun-ſhine. And of all leaves ſuch as are greyiſh, hairy, and of a dryiſh nature, are the moſt phoſphoric: and ſo is that ſide of the leaf that is next the ground. If theſe bodies do not ſhine by the ſun's rays, they always will by a lens: which is likewiſe of great ſervice in making flowers ſhine: for many of theſe, although their colour be not very dark, are yet more hard to make ſhine even than leaves. Fruits that are even ſoft and juicy are not very difficult; but dried ones are much eaſier.

25. The kernels of fruit, though not dried, and much leſs when roaſted, are good phoſphori. Theſe methods do indeed improve their light, but they will ſhine without them. In this particular I have changed my opinion very much; for I formerly thought that not only fruit and kernels, but that meal and the whiteſt ſtarch, and almoſt all ſeeds, were not naturally phoſphori: but would become ſuch artificially, that is, by moderate roaſting. But as I had not then my preſent accuracy of obſervation, and did not know the effect of a humid air in deſtroying theſe powers, I hope this miſtake will appear pardonable. Things appeared

peared to me quite different, when I returned to theſe obſervations this laſt time, which was in hot weather, a clear ſky, and a dry air: all which circumſtances were entirely different from thoſe that attended the firſt experiments. Therefore I now ſay, that nuts of all kinds, grains of wheat, pulſes, and in particular *peaſe* and *tares*, ſhine very brightly without any preparation; the two laſt mentioned are brighter than moſt other phoſphori.

26. I found that moſt vegetable juices were pretty ſtrong phoſphori: and that all ſhone a little. I formerly reckoned ſugar as a principal ſalt. Manna and honey are juſt as much inferior to ſugar in brightneſs as they are in drineſs. Honey ſcarce ſhines at all, unleſs it may be made hard, in ſome meaſure by cold, and ſo as to exhibit the appearance of grains. Gums, in which the whole genus of vegetable juices, that are not already mentioned, conſiſt, are very good phoſphori. Gum arabic, and likewiſe ſome drops of plum-tree gum, appeared very ſplendid: ſo will the tragacanth, provided it's texture be not looſened by humidity; and, if it is, it muſt be dried.

27. The laſt genus, which is that of the fat juices, is phoſphoric, though not in ſo great a degree as the other two. All reſinous diſtillations ſhine more or leſs: thoſe that are clear, white, and dry, are the beſt phoſphori; and the worſt are thoſe of an oppoſite nature. The reſin called benzoe could hardly be moved to ſhine by the lens: the ſame was obſerved of the turpentine reſin.

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This laſt would not ſhine whilſt in a liquid form; but ſhone a very little when hardened by cold.

28. We muſt reckon oils amongſt the fat juices. And if experiments made upon two ſorts of oil will warrant us to conjecture as to other ſorts, we may ſay that oils, whether they be preſſed from fruits, kernels, or ſeeds, or be diſtilled by a chymical fire, do all of them exhibit phoſphoric appearances. One of theſe oils was oil preſſed from olives: the other was diſtilled from anniſeed. Both of them ſhone by the ſun's rays only, but not till they were froze. From whence it might eaſily be ſuſpected that other oils would likewiſe ſhine in the ſame circumſtances.

29. At length we ſhall turn our diſcourſe to animals. It was ſaid in the former part that animals were as good phoſphori as foſſils or vegetables: but that this quality was confined to thoſe parts of animals which conſiſted entirely of an earthy principle, hardened into ſolidity. And that in other parts it was by the mixture of fat extinguiſhed, or at leaſt greatly weakened. But I find by my preſent experiments, that although this power be weakened by the oleaginous principle, yet it is not entirely extinguiſhed. For which reaſon thoſe parts of bodies which I have formerly called dark, are to be reckoned only as leſs ſplendid and not dark. By which means, whatever diſagreement may appear between the former obſervations and the preſent, will be eaſily reconciled. All parts of animals, therefore, do admit the light. Some with eaſe,

others

others with difficulty. Of the former ſort are ſuch parts as ſeem to grow out of bodies: as horns, hoofs, and hairs of quadrupeds; the feathers, beaks, and talons of birds: but the ſcales of fiſh, are faint phoſphori: though the ſhells of certain ſhell-fiſh are bright ones, as was obſerved before. I formerly imagined that the ſolenes, and the fins of fiſh, and the unguis odoratus, did not ſhine: but they are phoſphori, though faint ones. The hoof of an elk will ſhine, if the rays paſs through a lens, and fall upon the brighteſt parts of it.

30. Upon taking off the hairs and feathers of land-animals, and birds, the epidermis is obſerved to be phoſphoric. The hides of quadrupeds, as well raw as dreſt, and even though they be of deep colours, are phoſphori. So is the ſkin of birds, eſpecially in thoſe places that have leaſt fat, or are lifted up by the bones: for in thoſe places the ſkin is moſt lean. For it is a general rule, that all membranous and nervous parts, or rather whatever approaches to that glutinous nature, wherewith theſe animals, when living, abound, is very favourable to phoſphoric appearances. Inſtances may be given in teeth, bones, and horns, that are boiled up and reduced into the form of ice: likewiſe in cakes of portable meat ſoup: alſo in the concretion formed by the cerum of the blood, upon evaporating the aqueous part: and, laſtly, in that tough and yellowiſh coat that grows upon the blood; which is taken from patients in high inflammatory fe-

vers: for all theſe bodies, without the application of fire, are pretty ſtrong phoſphori. Though it is true, that this quality is improved by fire, yet it exiſts without it. If I had known this when I recommended the roaſting of theſe ſubſtances, I ſhould certainly have mentioned it, leaſt it might have been imagined, that becauſe I propoſed artificial means, that therefore theſe bodies were not naturally phoſphori.

31. The fleſh of animals ſeems to be the leaſt phoſphoric of all their parts. For which reaſon, in my firſt paper, I did not reckon any of them phoſphori, except white meats and thoſe roaſted. For I doubted ſo much with reſpect to brown meats, which colour I knew to be oppoſite to phoſphoriſm, that I paid no attention to them. And if I had obſerved them, as my apparatus was then ſo imperfect, I believe I could have made nothing of them: ſo languid and tranſient is even that light which they receive from the ſtrongeſt ſun-beams. It is otherwiſe as to bones. But in this reſpect I have nothing to add to what was before written. It is now ſhewn, that all the ſolid parts of animals are phoſphori. And if any of them ſhine with difficulty, this may be leſſened by drying them properly, as was before obſerved of vegetables.

32. With reſpect to animal juices, I ſhould imagine that they are all phoſphoric, ſince I have at length diſcovered that milk and fat are ſo, though I had formerly been of opinion, that milk

was obſcure, at leaſt ſo long as it continued fluid: and that fat extinguiſhed the phoſphoric qualities of bodies, or at leaſt weakened them. Whoever chuſes to try experiments upon milk, muſt make uſe of thoſe cautions before laid down with reſpect to experiments upon liquids in general: and with reſpect to animal fat, I ſhall content myſelf with the obſervations juſt now made upon vegetable oils: that is to ſay, they muſt be hardened by the cold: for if they receive the leaſt moiſture, their ſplendor will be entirely loſt.

33. Having found, by the foregoing obſervations and many others, which, to avoid prolixity, I have not mentioned, that this phoſphoric quality was ſo widely diffuſed amongſt animals, I began to conſider, whether living animals were endued with that power which I had ſeen in the different parts of dead ones. For I could ſee no reaſon why nature ſhould have denied this quality to living bodies, ſeeing that ſhe had beſtowed it upon ſuch as wanted ſenſe and life. But this imagination of mine, though it was ſupported with probability and analogy, yet it had not any clear and certain experiment to authorize it. Moreover, no ſuch obſervation could be made without changing my firſt method of obſerving, and making myſelf the object of my own experiment. I had ſubſtituted the double curtain in the place of my cylindrical tympanum, as before mentioned. By means of this contrivance, it was in my power to thruſt out my naked hand, or

arm, and to bring it back into the place of obſervation at pleaſure. It will be eaſily underſtood, that, in the common courſe of my experiments, I muſt have ſeen any ſplendor that might happen to remain upon my hand.

34. And in truth, when I firſt began to uſe my new apparatus, I ſaw, to my apprehenſion, ſome faint traces of light upon my fingers, eſpecially at the ſides and ends. I was pleaſed with the novelty of the appearance, but not ſo entirely poſſeſſed with it as to give immediate credit to my ſenſes. For which reaſon I carefully repeated the experiment, by expoſing ſometimes one part of my hand, and ſometimes another, to the light, by obſerving it in the dark with the utmoſt care, in order to diſcover whether the light would follow the various bendings of my fingers, or the ſhaking of the whole hand, by a conſtant adheſion to the ſame parts: in ſhort by comparing together, with the utmoſt exactneſs, the effect of all my obſervations, this diligence was very ſucceſsful, and I found my firſt conjecture confirmed.

35. But although the exiſtence of this light was quite certain, yet I found myſelf involved in a new queſtion; namely, whether the light reſided in the ſkin, or in ſome ſubſtance upon the ſkin; for I well knew that bodies which were of themſelves dark, will become phoſphori upon being rubbed with a very ſmall quantity of duſt; and will return to their original darkneſs, upon the

the duſt being wiped off. And I well remember that I had been very near determining, that ſome metallic maſſes were phoſphori, when I was miſled by ſuch an appearance. I knew too that the hands very eaſily contract dirt, by handling bodies. And although the ſkin be not made foul by foreign ſubſtances, yet it has in it's own texture, even in the niceſt perſon, a principle, by which it acquires filth. For it naturally becomes greaſy, by the perpetual effuſion of certain fat matter which ſometimes become dry, and aſſumes the form of ſcurf; which laſt ſubſtance I had before found to be phoſphoric. This made me ſuſpect, that this fat ſubſtance might ſhine when it lay upon the ſkin in a ſebatious form, although it was imperceptible to the ſight; and conſequently, that, if the human ſkin does ſhine, it is not by it's own light, but by that of another ſubſtance. This ſuſpicion was ſtrengthened by the entire extinction of the light, as often as the fingers, or the whole hand, were dipped in warm water; and, on the other ſide, this ſuſpicion was leſſened by conſidering the exquiſite cleanneſs of the hand before ſuch waſhing; and conſidering further, that this ſplendor was ſo vivid, and ſo widely diffuſed, that it ſeemed impoſſible to aſcribe it to ſo minute a quantity of dirt as was imperceptible to the ſight.

36. When I was in this uncertainty, and in great doubt, occaſioned by the arguments I have juſt now mentioned, it happened about the middle

dle of December laſt year, that my hand was obſerved to ſhine with an unuſual ſplendor; although, in the judgment of another obſerver, who was preſent, it appeared to be all over as clean as poſſible. I waſhed it ſeveral times; I wiped it, and expoſed it again to the ſun; and found it not at all, or a very little, impaired in it's brightneſs, which it retained even when wet. Both wet and dry it was often expoſed to the lens; on the back; on the hollow, which laſt had generally appeared dark; on the ball of the thumb; and, laſtly, on the tops of the fingers. All thoſe places ſhone very much, eſpecially the tops of the fingers, and of the thumb in particular: and it was obſerved, that the tops of the fingers, and ſome other parts of the hand, ſhone brighter for being preſſed cloſe. From which it was inferred, that the differences of the light, mentioned before, follow the different ſtates of the ſkin, and do not depend upon the accretion of any foreign ſubſtance: and certainly ſometimes the whole light was extinguiſhed by the ſimple immerſion of the hand in warm water; which was ſo ſlight that it could only ſoften the hand, but not waſh off any dirt that might be upon it; and upon many occaſions it has been obſerved, that ſoftneſs is oppoſite to the phoſphoric quality, and that dryneſs is favourable to it.

37. But at length a favourable ſeaſon cleared up this doubt, which my greateſt induſty had

ſerved only to increaſe and confirm. For in the beginning of this laſt January I returned to theſe experiments, and obſerved, that my whole hand, and the ſurface of my arm, were brighter than they had ever been before. For the places that had ſhone before were now brighter; and ſuch places as had been dark, now exhibited a conſiderable light. No external cauſe could be diſcovered. The hand and arm were perfectly clean. The cauſe therefore muſt be in the ſkin. But, upon the moſt diligent conſideration, there could be nothing unuſual that could affect the ſkin, except what aroſe from the extraordinary change which had happened in the weather: for at the time of theſe laſt experiments there was a moſt ſevere froſt; and every body knows, that the ſkin becomes rough, hard, pallid, and lean, in froſty weather; and it is as well known, that bodies with theſe qualities are diſpoſed to be phoſphoric. And thus this doubt was cleared, and the human ſkin proved upon good grounds to be phoſphoric.

38. And now we have gone through the ſeveral kingdoms of animals, vegetables, and foſſils of the viſible earth: and having by this means ſhewn the general prevalence of phoſphoric appearances in all bodies, there ſeems to be a natural end of what was at firſt propoſed. For I never propoſed more in theſe enquiries than to ſearch, as far as I was able, into theſe wonderful natural appearances, leaving their hidden cauſes to be diſcovered by perſons

ſons of better abilities. This laſt is the province of parts and ingenuity, in which reſpects I am ſenſible of my own weakneſs. Though I pretend to ſuch merit as may ariſe from induſtry and diligence. Nevertheleſs, in order to be as uſeful to philoſophers as may be, I intend to contract the obſervations hitherto made into a few general heads; to point out their connections, and their conſequences: to propoſe ſome conjectures, if I find room for them; to move ſome queſtions with a view of inducing others to ſpeculate upon theſe points: and laſtly, I ſhall inſert ſome new obſervations in order to ſtrengthen the foregoing, or to open the way to new ideas. And having gone through theſe points with all the diligence in our power, we ſhall put an end to this whole enquiry.

E R R A T A.

For Beccarii *read* Beccari.
Bolonia *read* Bologna.

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